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TRANSACTIONS

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ROYAL SOCIETY

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FOR THE YEAR MDCCCXXXVIII.

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APPENDIX.

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Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

Adjudication of the Medals of the Royal Society for the year 1838 by His Royal Highness the President and Council.

The Rumford Medal to James David Forbes, Esq., F.R.S., for his "Experiments on the Polarization of Heat," published in the Transactions of the Royal Society of Edinburgh.

A Copley Medal to Professor K. F. Gauss, Forn. Memb. R.S., for his "Inventions and Mathematical Researches on Magnetism."

Another Copley Medal to Michael Faraday, Esq., D.C.L., F.R.S., for his "Researches on Specific Electrical Induction."

The Royal Medal, in the department of Chemistry, to Thomas Graham, Esq., M.A., F.R.S., for his paper entitled "Inquiries respecting the Constitution of Salts, of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides," published in the Philosophical Transactions for 1836.

The Royal Medal, in the department of Mathematics, to William Henry Fox Talbot, Esq., F.R.S., for his papers entitled "Researches in the Integral Calculus," published in the Philosophical Transactions for 1836 and 1837.

PHILOSOPHICAL TRANSACTIONS.

IX. The Bakerian Lecture.—*On the Theory of the Astronomical Refractions.* By James Ivory, *K.H. M.A. F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

Received April 26,—Read May 3, 1838.

THE apparent displacement of the stars caused by the inflection of light in its passage through the atmosphere, is treated by the astronomer like most other irregularities which he has occasion to consider. A set of mean quantities is first provided; and the occasional deviations of the true places from the mean are ascertained and corrected according to the state of the air, as indicated by the meteorological instruments. The subject of the astronomical refractions is thus resolved into two parts very distinct from one another; the first embracing the mean refractions, which are an unchangeable set of numbers, at least at every particular observatory; the second relating to the temporary variations occasioned by the fluctuations which are incessantly taking place in the condition of the atmosphere. It is the first of these two questions chiefly, or that regarding the mean refractions, of which it is proposed to treat in this paper.

In order to form a just notion of the mean refractions, we may suppose that some particular star is selected, and assiduously observed for a course of time so considerable as to comprehend every possible change in the condition of the atmosphere; all these observed places being severally reduced to some assumed state of the thermometer and barometer, and being combined so as to eliminate occasional irregularities, will determine the mean refraction of the star. In this procedure it is supposed, what experience confirms, that the result will ultimately be the same for the same altitude above the horizon, provided the observations are numerous enough, and extend over a sufficient length of time. We may instance the star α Lyræ observed by Dr. Brinkley; his observations are forty-four in number, extending over five years; and the greatest deviation of single observations from the mean quantity may be stated at $\pm 20''$. The supplementary Table, extending from 85° to 89°½ of zenith distance, published in Bessel's *Tabulæ Regiomontanæ*, is one of mean refractions calculated from many

observations at every altitude. The Table, in the same work, extending to 85° of distance from the zenith, which the supplementary one is intended to complete, may likewise be considered as having the authority of actual observation; for although a theoretical formula was used in the calculations, yet the results have been carefully corrected by a comparison both with the observations of Bradley and with those made with very perfect instruments in the observatory over which Bessel presides. These two make together a table of mean refractions of the highest authority; and being free from hypothetical admissions, to speak with precision, they form the only table of the kind of which astronomy in its actual state can boast.

The mean refractions, being a fixed set of numbers at any proposed observatory, are independent of temporary changes in the state of the air. If the general constitution of the atmosphere were so well known as to enable us to deduce the temperature, the density, and the pressure at any given altitude, from the observed condition of the air at the earth's surface, it might be possible to pitch upon an atmosphere intermediate between the extreme cases, in which the irregularities would compensate one another. From such an atmosphere the mean refractions used in astronomy might be correctly computed. But in reality we have no exact knowledge of the variations to which the air is subject in ascending above the surface of the earth. The diffusion of heat and aqueous vapour, the laws which regulate the density and pressure, are but slightly and hypothetically known. Many laborious researches in the lower part of the atmosphere, to which access can be had with instruments, have not been attended with complete success; and they have thrown no light upon what takes place in the upper parts. The limit of the atmosphere, or the height at which the air ceases to have power to refract light, is uncertain, and is no doubt, as well as the figure of the limiting surface, subject to continual fluctuation. Reflecting on what is said, it must be evident that the mean refraction of a star, which is a fixed quantity, cannot possibly be deduced from an atmosphere daily and hourly varying in its essential properties.

A table of refractions, such as is used in astronomy, contains only mean effects of the atmosphere, that take place at a given point of the earth's surface; and they should properly be compared with other mean effects at the same place. Of these mean effects a principal one is the height that must be ascended in the air for depressing the thermometer one degree, from which another mean effect is easily deduced, namely, the rate at which the density of the air decreases as the height increases. The values of these quantities, as occasionally determined at any particular place, will vary according to the actual state of the air; but a multitude of particular determinations embracing every vicissitude of the atmosphere, will at length lead to mean quantities which are constant, and such as would be observed in the same atmosphere that produces the mean refractions.

It is found that the refractive power of air depends on the density to which it is proportional; and hence the rate at which the density varies at the earth's surface

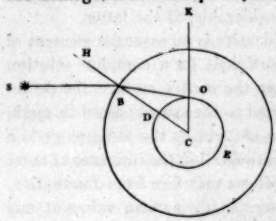
must have a great influence on the quantity of the astronomical refractions. It furnishes a key to the scale of the real densities in the atmosphere. When a thermometer is elevated in the air, it is found that the mercury continues to be depressed equably to great heights: in like manner the decrements of density will vary slowly from being proportional to the spaces passed through; so that a great share of that part of the astronomical refraction which depends upon the constitution of the atmosphere, must be ascribed to the initial rate at which the density decreases. This rate is not hypothetical; it is a real quantity independent of every other; its mean value, which alone we consider, is as determinate and as much the result of experiment as is the refractive power of the air: and in a solution of the problem which is not warped by arbitrary suppositions, and which deduces the effect only from causes really existing in nature, the former quantity will produce a part of the refraction as certain and unalterable, although perhaps not so considerable, as the latter.

But although the initial rate of the decrease of density is an essential element of the astronomical refractions, it may not alone be sufficient for a complete solution of the problem. In ascending to great heights above the earth's surface, the decrements of density will at length cease to be proportional to the spaces passed through, or to the variations of temperature. The refraction of light by the atmosphere is a complicated effect depending upon different considerations: but the influence of these considerations on the mean refractions must be uniform and free from fluctuation, and can arise only from quantities which are constant in their mean values at any proposed observatory. In speaking of mean quantities we exclude whatever is hypothetical, and confine our attention to such only as have a real existence in nature, although it may not in all cases be possible to obtain exact measurements by direct observation. As the refractions themselves are capable of being determined experimentally, they may be made the means of ascertaining what is left unknown in the formula for computing them; and they may thus contribute indirectly to advance our knowledge of the constitution of the atmosphere.

In proceeding to treat of this problem according to the notions that have been briefly explained, it remains to add that the mean effects of the atmosphere at the same observatory (of which mean effects a table of refractions is one) are alone considered, without at all entering on the question whether such effects are different or not, at different points of the earth's surface. It is very well known that the refractions, to a considerable distance from the zenith, depend only on the refractive power of the air and the spherical figure of the atmosphere: so far there is no reason to doubt that they are the same over a great part of the surface of the globe, according to the opinion generally held by astronomers: but, at greater zenith distances, when the manner in which the atmosphere is constituted comes into play, it is not so clear that they may not be subject to vary in different climates, and at different localities of the same climate. If a table of refractions at a given observatory contain a set of fixed numbers, these must be deducible from quantities not liable to change, that is,

from certain mean effects produced by the atmosphere at the observatory. To trace the relations that necessarily subsist between the mean effects that take place at a given point on the surface of the earth, is the proper business of geometry: if this can be successfully accomplished, the astronomical refractions will be made to depend upon a small number of quantities really existing in nature, and which can be determined, either directly or indirectly, by actual observation.

1. The foundation of the theory of the astronomical refractions was laid by Dominique Cassini. The earth being supposed a perfect sphere, he conceived that it was environed by a spherical stratum of air uniform in its density from the bottom to the top. By these assumptions the computation of the refractions is reduced to a problem of the elementary geometry requiring only that there be known the height of the homogeneous atmosphere, and the refractive power of air. Let the light of a star S fall upon the atmosphere at B, from which point it is refracted to the eye of an observer at O on the earth's surface DOE: the centre of the earth being at C, draw the radii COK, CDBH: the angle $KOB = \theta$, is the apparent zenith distance of the star; and $OBC = \varphi$ is the angle in which the light of the star is refracted on entering the atmosphere: now from the triangle OBC we deduce



$$\sin OBC = \sin KOB \times \frac{CO}{CB};$$

or, which is the same thing, putting $i = \frac{DB}{CD}$,

$$\sin \varphi = \frac{\sin \theta}{1 + i}.$$

Again, φ being the angle in which the light of the star is refracted, if we put $\delta\theta$ for the refraction, the angle of incidence SBH, which in the present case is always greater than the angle of refraction, will be $= \varphi + \delta\theta$; and $\frac{\sin(\varphi + \delta\theta)}{\sin\varphi}$ will be a constant ratio represented by $\frac{1}{\sqrt{1 - 2a}}$; so that

$$\sin(\varphi + \delta\theta) = \frac{\sin\varphi}{\sqrt{1 - 2a}} = \frac{\sin\theta}{(1 + i)\sqrt{1 - 2a}}.$$

Thus we have the two following equations, which furnish a very easy rule for computing the mean refractions according to Cassini's method, viz.

$$\sin \varphi = \frac{\sin . \theta}{1 + i}$$

$$\sin(\varphi + \delta\theta) = \frac{\sin\theta}{(1 + i)\sqrt{1 - 2a}}.$$

As i and α are both very small numbers, if we put

$$m = i - i^2,$$

$$n = i - \alpha - i^2 + \alpha i - \frac{3\alpha^2}{2},$$

the two last equations will become

$$\sin \varphi = \sin \theta - m \sin \theta,$$

$$\sin (\varphi + \delta\theta) = \sin \theta - n \sin \theta:$$

and by employing the usual formula for deducing the variation of the arc from the variation of the sine, we get

$$\varphi = \theta - m \tan \theta + \frac{m^2}{2} \tan^3 \theta,$$

$$\varphi + \delta\theta = \theta - n \tan \theta + \frac{n^2}{2} \tan^3 \theta:$$

consequently

$$\delta\theta = (m - n) \tan \theta - \frac{m^2 - n^2}{2} \cdot \tan^3 \theta;$$

that is,

$$\delta\theta = \left(\alpha - i\alpha + \frac{3\alpha^2}{2}\right) \tan \theta - \left(i\alpha - \frac{\alpha^2}{2}\right) \tan^3 \theta;$$

or, which is the same thing,

$$\delta\theta = \alpha \tan \theta \left(1 + \alpha - \frac{i - \frac{1}{2}\alpha}{\cos^2 \theta}\right) = \alpha (1 + \alpha) \tan \theta \left(1 - \frac{i - \frac{1}{2}\alpha}{\cos^2 \theta}\right),$$

agreeing exactly with LAPLACE's formula employed in computing the first part of the Table of mean refractions published by the French Board of Longitude.

2. The publication of NEWTON's Principia enabled geometers to take a more enlarged view of the astronomical refractions, and one approaching nearer to nature. According to CASSINI, the atmosphere is a spherical stratum of air, uniform in its density throughout, diffused round the earth to the height of about five miles; in reality the density decreases gradually in ascending, and is hardly so much attenuated as to be ineffective to refract the light at the great elevation of fifty miles. The path described by the light of a star in its passage through the atmosphere is therefore not a straight line, as it would be in the hypothesis of CASSINI, but a curve more and more inflected towards the earth's centre by the successive action of air of increasing density. Now in the Principia there is found whatever is necessary for determining the nature of this curve, and consequently for solving the problem of the astronomical refractions, which consists in ascertaining the difference between the direction of the light when it enters the atmosphere, and its ultimate direction when it arrives at the earth's surface. In the last section of the first book of his immortal work, NEWTON teaches in what manner the molecules of bodies act upon the rays of light and refract them; and as the atmosphere must be uniform in its condition at all equal altitudes, its action upon light can only be a force directed to the centre of the earth; so that

the trajectory in which the light moves, being described by a centripetal force, the determination of its figure will fall under the propositions contained in the second section of the same book.

Conceive that light falls upon an atmosphere A G K, constituted as Cassini supposed, spherical in its form, concentric to the earth, of the same density ϱ throughout; and suppose that the attractive force of the molecules of air situated in the surface A G K extends to $m\ n$ on one side, and to $m'\ n'$ on the other. Every molecule of light when it arrives at $m\ n$ will be attracted by the air in a direction perpendicular to the surface A G K, and tending to C the centre of the earth; it will continue to suffer a varied attraction till it penetrates to the other surface $m'\ n'$; but when it has passed this limit, it will no longer be acted upon effectively by the surrounding air, which will attract it equally in all opposite directions. As the attraction of air extends only to insensible distances, in estimating its action upon a molecule of light we may consider the limiting surfaces $m\ n$ and $m'\ n'$ as parallel planes, the forces being perpendicular to $m\ n$, and of the same intensity at all equal distances from it. The law of the forces in action between $m\ n$ and $m'\ n'$ is indetermined; it may be uniform, or varied in any manner. These things being premised, it follows from a fundamental proposition of the philosophy of Newton, the demonstration of which it would be useless to repeat here, that the total action of all the forces between $m\ n$ and $m'\ n'$ is to add to the square of the velocity of the light incident at $m\ n$, an increment which is always the same, whatever be the direction in which the light arrives at $m\ n$. If we now put v for the velocity with which the light enters $m\ n$, and v' for the velocity with which it leaves $m'\ n'$, what is said will be expressed by this equation,

$$v'^2 - v^2 = 2 \cdot \varphi(\varrho),$$

$\varphi(\varrho)$ denoting the sum of all the forces between $m\ n$ and $m'\ n'$, each multiplied by the space through which it acts, a sum which, in different atmospheres, will vary only when ϱ varies.

It will be convenient to have a name for the function $\varphi(\varrho)$, and the most appropriate term seems to be, the refractive power of the air. In using this term, or in expressing by $\varphi(\varrho)$ the action of air upon light, it is always supposed that the light passes out of a vacuum into air of the density ϱ.

A property resulting from what is said may be mentioned. Having drawn a radius from the centre of the earth to the point at which the light falls upon the atmosphere, let ϖ denote the angle made by the direction of the velocity v with the radius, and ϖ' the angle made by the direction of the velocity v' with it; then $v \sin \varpi$ and $v' \sin \varpi'$ will be the partial velocities of the light parallel to the surface of the atmosphere. Now these are equal; for all the forces which change v into v' are perpendicular to

the surface of the atmosphere, and therefore they have no effect to alter the velocity of the light parallel to that surface. Thus

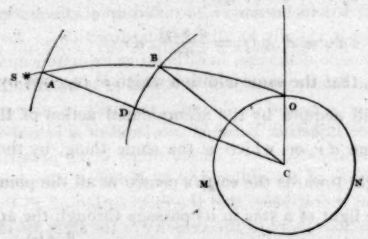
$$v \sin \varpi = v' \sin \varpi',$$

and

$$\frac{\sin \varpi}{\sin \varpi'} = \frac{v'}{v},$$

that is, in words, the ratio of the sine of incidence to the sine of refraction is equal to the ratio of the velocity of the light after refraction to the velocity of the incident light; which ratio, being independent of the direction of the incident light, is constant for all light that falls upon the atmosphere with the same velocity.

What has been said of an atmosphere supposed homogeneous is next to be applied to the real atmosphere of the earth, the density of which decreases continually in



ascending. The sphere M O N of which C is the centre, representing the earth, let S A B O be the trajectory described by light emanating from the star S, in its passage through the atmosphere to the earth's surface at O: through any two points of this curve, A and B, draw spherical surfaces concentric to the earth, the distances A C and D C from the common centre being $r + dr$ and r. Representing by ϱ the density of the air above the spherical surface at A, let $\varrho + d\varrho$ stand for the density, supposed uniform, of the stratum between the two surfaces at A and B: and it is to be observed that, though A D $= dr$ is an infinitesimal, it is nevertheless to be accounted infinitely great when compared to the insensible distance at which the molecular action of the air at A ceases to act: from which it follows that the refractive power of the stratum upon light which enters at A, is exactly equal to the refractive power of a homogeneous atmosphere, snpposing the density $\varrho + d\varrho$ to extend unvaried to the earth's surface. Now if v denote the velocity with which the light moves in the trajectory at A, the refractive power of the air above the stratum will diminish v^2 by the quantity $2\varphi(\varrho)$; for it is obvious that the refractive power of the air above the spherical surface at A, is equal and opposite to the refractive power of a homogeneous atmosphere within the same surface and of the density ϱ: on the other hand the refractive power of the stratum will augment v^2 by the quantity $2\varphi(\varrho + d\varrho)$:

wherefore, upon the whole, the real increment of v^2 will be $2\varphi(\varrho + d\varrho) - 2\varphi(\varrho)$; so that we shall have

$$d.v^2 = 2\varphi(\varrho + d\varrho) - 2\varphi(\varrho) = 2\,d.\varphi(\varrho);$$

and, by integrating,

$$v^2 = n^2 + 2\varphi(\varrho).$$

It is obvious that n^2 is the square of the velocity of the light before it arrives at the atmosphere, that is, when it moves in a vacuum. We may consider n^2 as the unit in parts of which the squares of the velocity of the light at the several points of the trajectory are estimated; which requires that the formula be thus written,

$$v^2 = 1 + 2\varphi(\varrho).$$

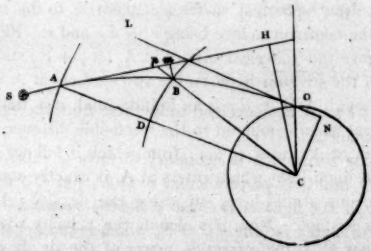
Resuming the equation

$$d.v^2 = 2\,d.\varphi(\varrho),$$

we have

$$v\,dv = d.\varphi(\varrho) = \frac{d.\varphi(\varrho)}{dr}.dr;$$

from which we learn, that the same addition which v^2 receives by the refractive power of the air at A, it will acquire by the accumulated action of the force $\frac{d.\varphi(\varrho)}{dr}$ at all the points of the line dr, or, which is the same thing, by the action of the force $\frac{d.\varphi(\varrho)}{dr}$ urging the light towards the earth's centre at all the points of the curve A B. Thus the path of the light of a star in its passage through the atmosphere is a trajectory described by the action of the centripetal force $-\frac{d.\varphi(\varrho)}{dr}$ tending to the centre of the earth, the sign $-$ being necessary, because the analytical expression is essentially negative.



Draw A H a tangent of the curve at A, B m perpendicular to A H, and produce C B to meet A H in p: put n for the angle A C O which the radius vector A C makes with C O the vertical of the observer; dz for A B the element of the curve; dv for the time of moving through A B; and R for the radius of curvature

at A. Now B p is the space through which the centripetal force $-\frac{d.\varphi(\varrho)}{dr}$ would cause a molecule of light to move from a state of rest in the time $d\tau$: wherefore

$$2\,\mathrm{B}p = -\frac{d.\varphi(\varrho)}{dr}\cdot d\tau^2;$$

also

$$\mathrm{B}p = \frac{\mathrm{B}m}{\sin \mathrm{B\,A\,D}} = \frac{dz^2}{2\mathrm{R}}\cdot\frac{dz}{r\,dn};$$

and, by equating the equal quantities, we get

$$\frac{dz}{\mathrm{R}} = -\frac{d.\varphi(\varrho)}{dr}\cdot\frac{d\tau^2}{dz^2}\cdot r\,dn. \qquad \ldots\ldots\ldots\ldots \quad (1.)$$

The refraction of the light in moving from A to B, or the difference of the directions of the curve at A and B, is evidently equal to the angle subtended by A B at the centre of the circle of curvature, that is, to $\frac{dz}{\mathrm{R}}$: wherefore if $\delta\theta$ represent the refraction increasing from the top of the atmosphere to the earth's surface, we shall have

$$d.\delta\theta = -\frac{d.\varphi(\varrho)}{dr}\cdot\frac{d\tau^2}{dz^2}\cdot r\,dn.$$

This formula is merely an application of the 6th proposition of the first book of the Principia.

Another general and useful expression of the differential of the refraction is easily obtained. Draw C H $= y$, perpendicular to the tangent A H: from the known properties of curve-lines, we have

$$\mathrm{R} = \frac{r\,dr}{dy};$$

wherefore

$$\frac{dz}{\mathrm{R}} = dy\cdot\frac{dz}{dr}\cdot\frac{1}{r} = \frac{dy}{\sqrt{r^2-y^2}};$$

consequently

$$d.\delta\theta = \frac{dy}{\sqrt{r^2-y^2}}, \qquad \ldots\ldots\ldots\ldots\ldots \quad (2.)$$

but in this formula $\delta\theta$ must be conceived to increase from the surface of the earth to the top of the atmosphere.

In applying the last formula it is necessary to have a value of y. Draw O L to touch the curve at O, and C N perpendicular to O L: put ϱ', v' for the density of the air, and the velocity of the light at O; also y' for the perpendicular C N, a for C O the radius of the earth, and θ for angle C O N, which is the apparent zenith-distance of the star: we shall have

$$\frac{\text{Area A B C}}{d\tau} = \frac{dz}{d\tau}\times y = v\times y;$$

and because the curve is described by a centripetal force tending to C, the value of

$v \times y$ will be the same at all the points of the curve; wherefore

$$v \times y = v' \times y';$$

and

$$y = y' \times \frac{v'}{v} = a \sin \theta \times \frac{v'}{v}.$$

Now, according to what was before shown,

$$v = \sqrt{1 + 2\varphi(\varrho)},$$

$$v' = \sqrt{1 + 2\varphi(\varrho')};$$

wherefore

$$y = a \sin \theta \times \sqrt{\frac{1 + 2\varphi(\varrho')}{1 + 2\varphi(\varrho)}}. \quad \ldots\ldots\ldots\ldots \quad (3.)$$

By substituting this expression in the differential of the refraction, the problem will be reduced to an integration.

The equations that have been investigated are perfectly general, and will apply in any constitution of the atmosphere that may be adopted. It has been thought better to consider the manner in which the forces act, than to employ functions with peculiar properties to express the molecular action. When the light in passing through the atmosphere arrives at a surface of increased density, it receives an impulse which may be considered as instantaneous; and this impulse being distributed over the breadth of a stratum of uniform density, ascertains the centripetal force tending to the earth's centre, by the action of which the trajectory is described.

3. It appears that Newton himself was the first to apply this new method to the problem of the astronomical refractions. A table, which he had computed, and which he gave to Dr. Halley, is published in the Philosophical Transactions for 1721. Nothing is said as to the manner in which the table was constructed: and it has always been a curious and interesting question among astronomers, whether it is the result of theory, or is deduced from observations alone, without the aid of theory. Some original letters of Newton to Flamsteed, published in 1835 at the expense of the Board of Admiralty, have put an end to all doubts on this point. These letters prove that Newton studied profoundly the problem of the refractions; that for several months in succession he was occupied almost entirely in repeated attempts to overcome the difficulties that occurred in this research; during which time he calculated several tables with great labour, namely, the one he gave to Halley, and another, or rather three others, which have been found in his letters to Flamsteed lately printed.

Admitting that the refractive power of the air is proportional to its density, which Newton had established in his Optics on speculative principles, and which Hawksbee afterwards was the first to demonstrate experimentally, the mathematical solution of the problem requires a knowledge of the law according to which the densities vary in the atmosphere. In his first attempt Newton assumes that the densities decrease in ascending in the same proportion that the distances from the earth's centre increase. Now a and r denoting the same things as before, put l for the total height

of the atmosphere; then $\varphi(\varrho)$ the refractive power of the air at the distance r from the centre of the earth, will, according to this hypothesis, be expressed by the formula

$$\varphi(\varrho) = \varphi(\varrho') \times \frac{l - r + a}{l}.$$

If this value be substituted in the formula (1.), which is a deduction from the sixth proposition of the first book of the Principia, the result will be

$$d.\delta\theta = \frac{\varphi(\rho')}{l} \cdot \frac{d\tau^2}{dx^2} \cdot r\, dn.$$

In this expression we have

$$\frac{d\tau^2}{dx^2} = \frac{1}{v^2} = \frac{1}{1 + 2\varphi(\rho)};$$

and as $2\varphi(\varrho)$, or the increment of the square of the velocity of the light is very minute, amounting to less than ·0006 in passing through the whole atmosphere to the earth's surface, we may reckon $\frac{d\tau^2}{dx^2}$ as unit; thus we get

$$d.\delta\theta = \frac{\Phi(\rho')}{l} \cdot r\, dn;$$

and by integrating

$$\delta\theta = \frac{2\Phi(\rho')}{\left(\frac{l}{a}\right)} \cdot \int \frac{r\, a\, dn}{2a^2}.$$

This result, which M. Biot has also obtained, is equivalent to the geometrical construction communicated by Newton to Flamsteed in a letter from Cambridge, December 20, 1694. The problem was now reduced to the quadrature of a curve, for which a general method is given in the fifth lemma of the third book of the Principia, a method which is still used when the direct process of integration fails, or becomes too intricate for practice. What has been said not only proves the exactness of Newton's solution of the problem; it also points out, with little uncertainty, the manner in which he obtained it. Of the arithmetical operations of the quadrature there is no account; and they would be of no interest had they been preserved. He complains much of the great labour of the numerical calculations; but all difficulties were overcome, as was to be expected: a table was computed and communicated to Flamsteed in a triple form, for summer, winter, and the intermediate seasons of spring and autumn. On mature reflection there occurred to him a serious objection to the supposed scale of densities, on which account he writes to Flamsteed that he does not intend to publish the tables. The fault lies in this, that the centripetal force which continually inflects the light to the earth's centre, is the same at all the points of the trajectory, or, in the words of Newton, the refractive power of the atmosphere is as great at the top as at the bottom,—than which nothing certainly can be more different from what actually takes place in nature.

Dismissing his first hypothesis, Newton next turned his attention to the 22nd proposition of the third book of his Principia. If the atmosphere consist of an elastic fluid gravitating to the earth's centre in the inverse proportion of the square of the distance, and if it be admitted that the densities are proportional to the pressures, Newton, in the proposition cited, proves in effect that the densities will form a decreasing geometrical series, when the altitudes are taken in arithmetical progression*. He writes to Flamsteed that an atmosphere so constituted is *certainly the truth*. Newton evidently intended by this assertion to mark a distinction between pressure, which is a cause of the variation of density that actually exists in nature, and his first assumed law of the densities, which is entirely arbitrary. Setting aside hypothesis, he now advanced so far in the true path of investigation; and if the manner in which heat is diffused in the atmosphere and the consequent decrease of density were not known when he wrote, he advanced as far as the existing state of knowledge enabled him to do. It is certain from his letters, that, after much time and labour, he at last succeeded in calculating a table of refractions on the principle that the density is proportional to the pressure. Such a table he communicated to Flamsteed, although it is not found in the letters lately published; and there is every reason to think it the same which he gave to Halley, and which that astronomer inserted in the Philosophical Transactions for 1721. Two elements only are sufficient for computing all the numbers in a table of refractions constructed by assuming that the density is proportional to the pressure, namely, the refraction at 45° of altitude, and the height of the homogeneous atmosphere, which is deducible from the horizontal refraction. The table of Halley, therefore, contains in itself all that is required for ascertaining whether it was calculated or not by the principle alluded to in the letters of Newton to Flamsteed. Kramp seems to be the first who sought in the table for the manner of its construction; and his discoveries in this branch of science enabled him to assign the height of the homogeneous atmosphere, which is one essential element. The refraction at 45° of altitude, which is the other element, is found in the table equal to 54″, or, in parts of the radius, to ·0002618; and Kramp found 4377† toises for l, the height of the homogeneous atmosphere; so that, if a be the radius of the earth in toises, we have

$$\alpha = \cdot 0002618,$$

$$i = \frac{l}{a} = \cdot 0013356;$$

and the two elements, α and i, are sufficient for computing the whole table, if it be

* Newton demonstrates strictly that the densities will be in geometrical proportion when the distances from the earth's centre are in musical or harmonical proportion, that is, when they are the reciprocals of an arithmetical progression; but in a series of this kind, if the first term bear an almost infinitely great proportion to the differences of the following terms, as is the case of the radius of the earth when compared to elevations within the limits of the atmosphere, the differences of the terms or the elevations may, without sensible error, be reckoned in arithmetical progression.

† Anal. des Réfractions Astronomiques, p. 19.

such as is mentioned in the correspondence between NEWTON and FLAMSTEED. The formula for the refraction in the supposed constitution of the atmosphere has been given both by KRAMP and LAPLACE; and it may be taken from the Paper in the Philosophical Transactions for 1823, p. 441,

$$\lambda = \frac{\alpha}{i} = \cdot 19601, \quad \Delta = \sqrt{\cos^2\theta + 2is},$$

$$\delta\theta = \alpha \sin\theta \times \left\{ \left(1 - \lambda + \frac{\lambda^2}{2} - \frac{\lambda^3}{6}\right) . \int \frac{dsc^{-s}}{\Delta} \right.$$
$$+ (\lambda - 2\lambda^2 + 2\lambda^3) . \int \frac{2\,dsc^{-2s}}{\Delta}$$
$$+ \left(\frac{3}{2}\lambda^2 - \frac{9}{2}\lambda^3\right) . \int \frac{3\,dsc^{-3s}}{\Delta}$$
$$\left. + \frac{8\lambda^3}{3} . \int \frac{4\,dsc^{-4s}}{\Delta}, \right.$$

the integrations extending from $s = 0$ to $s = \infty$. The coefficients of this formula are as follows:

$$A = 1 - \lambda + \frac{\lambda^2}{2} - \frac{\lambda^3}{6} = \cdot 82193$$

$$B = \lambda - 2\lambda^2 + 2\lambda^3 = \cdot 13423$$

$$C = \frac{3}{2}\lambda^2 - \frac{9}{2}\lambda^3 = \cdot 02377.$$

$$D = \frac{8}{3}\lambda^3 = \cdot 02007.$$

For the horizontal refraction, when $\cos\theta = 0$, $\Delta = \sqrt{2is}$, we obtain by the usual integrations,

$$\delta\theta = \frac{\alpha\sqrt{\infty}}{\sqrt{2i}} \times \left\{ A + B\sqrt{2} + C\sqrt{3} + D\sqrt{4} \right\};$$

or in seconds, $\delta\theta = 2024''2$ instead of $2025''$ as in HALLEY's table. This proves the exactness of KRAMP's elements.

With respect to the other numbers in the table a distinction must be made. In every table of refractions, whatever be the constitution of the atmosphere on which it is founded, the numbers answering to altitudes greater than 16°, depend only upon one element, namely, the refractive power of the air. Reckoning from the zenith as far as 74°, any table may be deduced from any other, provided both are accurately calculated, merely by a proportion. In the table published annually in the *Con. des Temps*, the refraction at 45° is $58''2$: and, if HALLEY's table has been accurately computed, the numbers in it, between the limits mentioned, will be equal to the like numbers in the French table multiplied by $\frac{540}{582} = \frac{90}{97}$. The calculation being made,

the results will be found to agree almost exactly with the short table inserted by M. Biot in the additions to the *Con. des Temps* for 1839, p. 105, the greatest difference between the computed quantities and the numbers in Halley's table being about $2''$.

But this gives no intimation with respect to the particular constitution of the atmosphere assumed in the calculation of the table. What is peculiar to a table in this respect has no sensible influence on the refractions it contains except at altitudes less than 16°. No definitive opinion can therefore be formed on the question, whether Halley's table is the same which Newton computed and communicated to Flamsteed on the principle that the densities are proportional to the pressures, without comparing it with a sufficient number of refractions at low altitudes calculated from the elements of Kramp. As the settling of this point may be thought not unimportant, the following formula, which affords the means of computing the refractions at all altitudes with exactness, has been investigated by reducing the integrals in the expression of $\delta\theta$ to serieses.

$$\text{Tan}\,\varphi = \frac{2\sqrt{3i}}{\cos\theta}, \quad e = \tan\frac{\varphi}{2}.$$

$$\text{Log tan}\,\varphi = \log\text{secant}\,\theta + 19{\cdot}2133569 - 20.$$

$$\begin{aligned}
\delta\theta = \sin\theta \times \Big\{ & e \times 660''{\cdot}795 \ldots . \overset{\log.}{2{\cdot}8200669} \\
& + e^3 \times 551{\cdot}634 \ldots . 2{\cdot}7337059 \\
& + e^5 \times 371{\cdot}268 \ldots . 2{\cdot}5696873 \\
& + e^7 \times 219{\cdot}762 \ldots . 2{\cdot}3419630 \\
& + e^9 \times 116{\cdot}763 \ldots . 2{\cdot}0673034 \\
& + e^{11} \times 58{\cdot}170 \ldots . 1{\cdot}7646976 \\
& + e^{13} \times 28{\cdot}275 \ldots . 1{\cdot}4514092 \\
& + e^{15} \times 13{\cdot}797 \ldots . 1{\cdot}1397974 \\
& + e^{17} \times 6{\cdot}806 \ldots . 0{\cdot}8329041 \\
& + e^{19} \times 3{\cdot}311 \ldots . 0{\cdot}5199046 \Big\}
\end{aligned}$$

The series converges very slowly, which has made it necessary to continue it to ten terms, the amount of which is still $3''{\cdot}6$ deficient from the exact quantity $2024''{\cdot}2$. As the last terms decrease in the proportion of 2 to 1, it is obvious that the true sum would be obtained by continuing the series: but the terms set down are more than sufficient for the present purpose.

The exact refractions calculated by the formula are next to be compared with the numbers in HALLEY's table.

Apparent zenith-dist.	Refractions		Difference.
	Computed.	HALLEY's Table.	
°	′ ″	′ ″	″
20	19·6	20	−0·4
40	45·2	45	+0·2
60	1 33·1	1 32	+1·1
70	2 27·0	2 26	+1·0
80	4 55·2	4 52	+3·2
82	6 3·9	6 00	+3·9
84	7 45·0	7 45	0·0
86	10 53·1	10 48	+5·1
87	13 22·5	13 20	+2·5
88	17 4·6	17 8	−3·4
89	23 3·5	23 7	−3·5

The examination that has now been made fully establishes that HALLEY's table is no other than the one which NEWTON computed on the supposition that the densities in the atmosphere are proportional to the pressures. The discrepancies, amounting in every instance to a small part of the whole quantity, are such as might be expected to occur unavoidably in the intricate and laborious methods of calculation followed by NEWTON. Some part of the differences may also arise from the elements of the formula, which, being deduced from only two numbers of the table, may not exactly coincide with the fundamental quantities which NEWTON assumed. M. BIOT, by a method different from KRAMP's, has found other elements, which make the horizontal refraction 8″·3 less than in the table; but these small variations, which proceed from calculating in different ways, only confirm more strongly that HALLEY's table is the same which NEWTON constructed by his second hypothesis, and communicated to FLAMSTEED.

It appears from what has been said, that, as far as the mathematics is concerned, the problem of the astronomical refractions was fully mastered by NEWTON. It would be strange indeed to suppose that the author of the theories in the Principia would find difficulty to apply them in a case to which he bent the whole force of his mind. But he was embarrassed by the suppositions he found it necessary to make respecting the physical constitution of the atmosphere. His first hypothesis is evidently contrary to nature, in admitting that air at all altitudes exerts the same power to inflect the light to the earth's centre; his second method made the refractions too great near the horizon, thereby proving the necessity of searching out some new cause for the purpose of reconciling the theory with observation. Averse from hypotheses, he seems, on these accounts, to have declined inserting in his works a problem which had cost him so much labour, and upon his solution of which he evidently set some value.

If the different attempts to solve this problem are to be tried with the same rigour

that Newton judged his own, it must be decided that they are all liable to objections. They all involve some supposition that has no foundation in nature; or they leave out some necessary condition of the problem. It is allowed that the variation of heat at different altitudes is unknown; that we are equally unacquainted with the manner in which is diffused the aqueous vapour that is always found, more or less, in the atmosphere; that the law of the densities has not been ascertained. But besides these capital points, the accurate M. Poisson will suggest other properties that must be attended to in an atmosphere in equilibrium: the conducting power of heat varying with the condition of the air; its power of absorbing heat; and the interchange by radiation which takes place with the earth, with the etherial spaces above, and with the stars visible above the horizon. So many conditions, placed beyond the reach of our inquiry, may well puzzle the most expert algebraist to take them into account. But it may be doubted whether this be the proper view of the problem. The astronomical refractions at any observatory are mean effects of the atmosphere; and it may be alleged that the proper way of accounting for them is to compare them with other mean effects of the atmosphere at the same place.

4. In 1715, twenty years after the researches of Newton, Brook Taylor published in his *Methodus Incrementorum*, the first investigation of the astronomical refractions on the supposition that the density of the air is variable. The differential equation is accurately deduced from the principles laid down by Newton; which removed all the difficulties of the problem in this respect.

Kramp, in his *Analyse des Réfractions Astronomiques et Terrestres*, has elucidated the elementary parts of the problem, and has greatly improved the mathematical solution. His method is particularly commodious and useful in the case of the horizontal refraction. For altitudes above the horizon the integrals are not susceptible of being simply expressed, and seem to require the aid of subsidiary tables in applying them.

The problem of the refractions being an important one in astronomy, many solutions of it have been published by different geometers. Some of these are preferable to others, because the method of calculation is easier in practice. For altitudes greater than 16°, they may all be reckoned equivalent, differing from one another only in the quantity assumed for the refractive power of air. They also mostly agree in the horizontal refraction, which is taken from observation. But for altitudes less than 16°, they are different: because, at these low altitudes, the refractions are affected by the arbitrary suppositions used in constructing the tables.

Thomas Simpson published a judicious dissertation on this problem. He distinctly points out that, to a considerable distance from the zenith, the refractions are independent of the manner in which the atmosphere is supposed to be constituted. In comparing the two atmospheres that have densities decreasing in arithmetical and geometrical progression, he remarks that the horizontal refraction comes much nearer the observed quantity in the first atmosphere than it does in the second; for which

reason he gives the preference to the first as likely to represent the phenomena with greater accuracy. Now in this his reasoning is not much different from the argument afterwards used by Laplace, to prove that the same two atmospheres are limits between which the true atmosphere is contained.

Newton likewise found that the refractions computed according to his second method, that is, in an atmosphere with densities decreasing in geometrical progression, are too great near the horizon, on which point he thus writes to Flamsteed. "Supposing the atmosphere to be constituted in the manner described in the 22nd proposition of my second book (which certainly is the truth), I have found that if the horizontal refraction be 34′, the refraction at the altitude of 3° will be 13′ 3″; and if the refraction in the apparent altitude of 3° be 14′, the horizontal refraction will be something more than 37′. So that instead of increasing the horizontal refraction by vapours, we must find some other cause to decrease it. And I cannot think of any other cause besides the rarefaction of the lower region by heat." Here the true reason is assigned why the refractions near the horizon, in an atmosphere constituted as supposed, so much exceed the observed quantities. When the density is made to depend solely on the incumbent weight, the air is not rarefied enough; and the greater density causes a greater refraction. Having correctly estimated the effect produced by the pressure of the supported air, Newton is unavoidably led to ascribe to heat the greater rarefaction that takes place in the atmosphere of nature. His words prove that he had no clear conception in what manner the density in the lower region is altered by the agency of heat; and, to say the truth, nearly the same ignorance in this respect prevails now as in his time. The decrease of density in ascending is a complicated effect of many causes for the most part unknown; and it seems in vain to expect a satisfactory investigation of it by arbitrary suppositions. But setting aside hypothetical constitutions of the atmosphere, we may consider the rarefaction of the air in ascending as a phenomenon, the knowledge of which is to be acquired by experiment; and this appears the only sure way of placing the theory of the mean refractions on its proper foundation.

5. One of the tables of refraction most esteemed by astronomers is that published annually in the *Con. des Temps*. It has been already shown that, as far as 74° from the zenith, this table is calculated by the simple method of Cassini. There is nothing incidental in this; for all tables of refraction may be computed by Cassini's method to the extent mentioned. The French astronomers have been very successful in determining the constants of the formula. The refractive power of the air was obtained by Delambre from a great number of astronomical observations; the same quantity was deduced by MM. Biot and Arago from experiments on the gases with the prism; and the results of two methods, so entirely different, agree so nearly, that there seems no ground for preferring one to the other. The remaining part of the French table, for altitudes less than 16°, is computed by a method of Laplace, which the author has explained, without disguising its defects, in the fourth volume of the *Mec. Celeste*.

The two atmospheres with densities decreasing in arithmetical and geometrical progression, which it now appears were imagined by Newton, and which have been discussed by Thomas Simpson and other geometers, are found, when the same elements are employed, to bring out horizontal refractions on opposite sides of the observed quantity. Laplace conjectured that an intermediate atmosphere which should partake of the nature of both, and should agree with observation in the horizontal refraction, would approach nearly to the true atmosphere. It must be allowed that these conditions, which may be verified by innumerable instances between the two limits, are vaguely defined; and in order to ascertain the real meaning of the author, recourse must be had to the algebraic expressions. When this is done it will be found that the atmosphere intended is one of which the density is the product of two terms, one taken from an arithmetical, and the other from a geometrical progression; the effect of which combination is to introduce a supernumerary constant, by means of which the horizontal refraction is made to agree with the true quantity. No one will deny the merit and the ingenuity of Laplace's procedure; but though very skilful, and guided in some degree by fact, it is liable to all the uncertainty of other arbitrary suppositions, as indeed the author allows. Dr. Brinkley has given the character of the French table fairly when he says, that it is only a little less empirical than the other tables. On divesting Laplace's hypothesis of vagueness in the language, and expressing it in the unequivocal symbols of algebra, it does not appear to possess any superiority over other supposed constitutions of the atmosphere in leading to a better and less exceptionable theory; at least the *Mec. Celeste* has been many years before the public, during which time not a few geometers have laboured on the subject of the refractions; but no improvement originating in the speculations peculiar to Laplace has occurred to any of them.

Having said so much on the theory of the French table, it may be proper to add a word on its accuracy. If it be compared from 80° to 88° of zenith distance with Bessel's observed refractions, there will be found a small error in excess, continually increasing, and amounting at last to 4″. This shows that, in combining the two atmospheres, too much weight has been given to that with a density varying in geometrical progression, in consequence of which the air is not rarefied enough in the interpolated atmosphere. With respect to the two degrees of altitude next the horizon, no accurate judgement can be formed, for want of observed refractions that can be depended on.

The astronomical refractions have also occupied the attention of the astronomer of Kœnigsberg, who has contributed so largely to the improvement of every part of astronomical science. For the purpose of representing the observations of Bradley with all the accuracy possible, Bessel investigated a table of refractions which appeared in the *Fundamenta Astronomiæ* in 1818. He assumes a theoretical formula; but as every arbitrary quantity is determined by a careful comparison with real observations, what is suppositious may be considered merely as an instrument of investigation, which is finally laid aside, leaving the result to rest on the foundation of

fact. He returned to the subject in his *Tabulæ Regiomontanæ*, published in 1830. In this last work he retains only that part of the table of 1818 which extends to 85° from the zenith, many corrections being applied from recent observations made with improved instruments. In order to supply what is wanting in the new table, BESSEL has added a supplemental one containing the refractions at every half degree for altitudes less than 15°: which supplemental table is independent of theory, being deduced from observations alone. These two tables form together a real table of mean refractions, independent of all suppositions respecting the constitution of the atmosphere; and no other similar table of nearly equal authority is to be found in the astronomy of the present day. What BESSEL has accomplished on the subject of the refractions is not the least important part of his labours for the advancement of astronomical science: it is precious to the practical astronomer; and it is necessary to the theoretical inquirer, for enabling him to confront his speculations with the phenomena to be accounted for.

6. In the paper published in the Philosophical Transactions for 1823, the refractions are deduced entirely from this very simple formula,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - f(1 - c^{-u}), \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4.)$$

in which β stands for the dilatation of air, or a gas, by heat; τ' is the temperature at the earth's surface, and τ the temperature at any height above the earth's surface; at the same height c^{-u} is the density of the air in parts of its density at the surface.

In order to understand the application of the formula, it is necessary to premise that in the remaining part of this paper we do not consider a variable atmosphere subject to continual fluctuations, as is the case of the real atmosphere: we contemplate an atmosphere fixed in its condition at any given place or observatory, being supposed a mean between all the variations that actually take place in an indefinite time. In such an atmosphere the temperature and pressure at the earth's surface will be mean quantities deduced from observation: the air at all elevations will have an elastic force equal to the incumbent weight which it supports, as an equilibrium requires: and, whether the air be dry or moist, its refractive power will be equal to the refractive power of dry air subjected to the same pressure and temperature*. These properties of the mean atmosphere rest upon experiment and demonstration: in other respects its nature is not directly known to us: and the laws of its action can only be discovered, not by hypothesis, but by observation.

The consideration of a mean atmosphere, invariable at any given observatory, is a necessary consequence of the notion we attach to the mean refractions; for these would be realized in such an atmosphere: but they are different in any other state of the air.

These observations being premised, if the formula (4.) be verified at the earth's

* Additions à la Conn. des Temps, 1839, p. 36.

surface in any invariable atmosphere, by giving a proper value to the constant f, it will still hold, at least with a very small deviation from exactness, at a great elevation, probably at a greater elevation than has ever been reached by man. In order to prove this, let the arbitrary function $\varphi(u)$ be added, so as to complete the formula by rendering it perfectly exact: then

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - f(1 - c^{-u}) - \varphi(u), \quad \ldots\ldots\ldots\ldots \quad (5.)$$

and it will follow that $\varphi(u) = 0$, when $u = 0$, that is, at the earth's surface. Again, differentiate the equation, observing that τ decreases when u increases, then

$$\frac{\beta}{1+\beta\tau'} \cdot \frac{d\tau}{du} = f c^{-u} + \frac{d.\varphi(u)}{du};$$

now, since this equation is true for all values of u, it will hold at the earth's surface, or when $u = 0$: and if f be taken equal to the particular value of

$$\frac{\beta}{1+\beta\tau'} \cdot \frac{du}{d\tau},$$

when $u = 0$, it will follow that $\frac{d.\varphi(u)}{du} = 0$, when $u = 0$. And since the equations $\varphi(u) = 0$ and $\frac{d.\varphi(u)}{du} = 0$, are both verified at the earth's surface, it follows that the supplementary function $\varphi(u)$ will vary slowly as u increases, that is, as the density of the air decreases in ascending. This proves that the approximate equation (4) will be very little different from the exact equation (5.) at great elevations in the atmosphere.

At the surface of the earth du is equal to the variation of the density for a depression of the thermometer expressed by $d\tau$: and although the proportion of these two quantities cannot be ascertained by direct experiment, yet, as is shown in the paper of 1823, it is easily deduced from the rate at which the temperature decreases as the height increases, which rate is easily determined experimentally. The quantity f thus found is necessarily constant at the same observatory. It is the mean of all the occasional values, which vary incessantly, while the real atmosphere undergoes every vicissitude of which it is susceptible. The mean refraction and f are invariable in quantity, because they depend alike upon the mean condition of the air at a given place. Some confusion has arisen on this point from not distinguishing between the mean refraction of a star and its true refraction in a variable atmosphere.

In all that has been said there is no supposition of an arbitrary constitution of the atmosphere. The assumed formula (4.) is an approximate truth in every invariable state of the air in equilibrium. Conceive a cylindrical column of air perpendicular to the earth's surface, and extending to the top of the atmosphere; at the height where the temperature is τ, and the density ϱ, let p denote the weight of the column above the height; and suppose that p, ϱ, τ are changed into p', ϱ', τ' at the surface of the

earth; because the pressures are proportional to the elasticities, we have the usual equation,

$$\frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \times \frac{\rho}{\rho'};$$

or, which is the same,

$$c^{-u} = \frac{\rho}{\rho'}, \quad \frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \times c^{-u};$$

and by substituting the complete expression of the temperature as given in (5.), we shall obtain,

$$\frac{p}{p'} = c^{-u} - f(c^{-u} - c^{-2u}) - c^{-u} \times \varphi(u); \quad . \quad . \quad . \quad . \quad (6.)$$

and if we omit the supplemental part, which is small even at great elevations, the result will be,

$$\frac{p}{p'} = c^{-u} - f(c^{-u} - c^{-2u}).$$

Now this is the constitution of the atmosphere in the paper of 1823; it is only approximate; but it is an approximation applicable to every atmosphere that can be imagined, requiring no more than that the quantity f have the proper experimental value given to it. It is shown in the paper that the pressures and densities are thus represented with no small degree of accuracy at the greatest heights that have been reached; which proves how slowly the supplemental part of the formula (5.) comes into play.

The foregoing manner of arriving at the constitution of the atmosphere adopted in the paper of 1823, being drawn from properties immediately applicable to the problem in hand, is more natural, and more likely to suggest itself, and more satisfactory than the ingenious and far-fetched procedure of M. Biot, of transforming an algebraic formula for the express purpose of bringing out a given result. Laplace, having remarked that the true horizontal refraction is contained between the like quantities of two atmospheres, with densities decreasing in arithmetical and geometrical progression, conjectured that an atmosphere between the two limits, which should likewise agree with observation at the horizon, would in all probability represent the mean refractions with considerable accuracy. It is upon this assumption that the problem is solved in the *Mec. Celeste*, the observed horizontal refraction being used for determining the arbitrary constant. Now in the paper of 1823 there is no allusion to interpolating an atmosphere between two others; a knowledge of the horizontal refraction is not required; the investigation is grounded upon a property common to every atmosphere in a quiescent state; and lastly, the resulting table is essentially different from all the tables computed by other methods. If these considerations be not sufficient to stamp an appropriate character upon the solution of a problem, it would be difficult to find out what will be sufficient. But if it be possible, with M. Biot's ingenuity, to trace some relation in respect of the algebraic

expressions, between the paper of 1823 and the calculations of LAPLACE, from which, after all, no just inference can be drawn, it is not difficult to find between the same paper and the view of the problem taken by the author of the Principia, in 1696, an anology much more simple and striking, which deserves to be mentioned as it tends to bring back the investigation to the right tract, which it seems to have left. NEWTON, having solved the problem on the supposition that the density of the air is produced solely by pressure, found that the refractions thus obtained greatly exceeded the observed quantities near the horizon: and hence he inferred, in the true spirit of research, that there must be some cause not taken into account, such as the agency of heat, which should produce, in the lower part of the atmosphere, the proper degree of rarefaction necessary to reconcile the theoretical with the observed refractions. Now, in the paper of 1823, the sole intention of introducing the quantity f, not noticed before by any geometer, is to cause the heat at the earth's surface to decrease in ascending at the same rate that actually prevails in nature; which evidently has the effect of supplying the desideratum of NEWTON.

The remarks that have just been made are not called for by anything which M. BIOT has written in his dissertation on the refractions, inserted in the additions to the *Conn. des Temps* for 1839; because that author has fully explained the grounds of what he advances, thereby enabling a candid inquirer to form his own opinion: but all the world are not of the same character as that distinguished philosopher.

At every point on the earth's surface we are now acquainted with three things, not hypothetical or precarious, that have an influence on the mean refractions. These are, the refractive power of the air, the spherical figure of the atmosphere, and the mean rate at which the density of the air decreases at the given place. These three things are independent on one another, and on all other properties of the air: they will therefore produce three independent parts of the quantity sought. The parts thus determined may fall short of the whole refraction at any altitude, because there may be causes not taken into account that co-operate in producing the result: but each will unalterably maintain its proper share of the total amount, in whatever way it is attempted to solve the problem, provided the solution is conducted on right principles and not warped by arbitrary suppositions. It may therefore be said that, in so far, an advance has been made in acquiring an exact notion of the nature of this problem.

The table in the paper of 1823 was compared with the best observations that could be procured at the time of publication; and the results were very satisfactory. After the publication of the *Tabulæ Regiomontanæ*, it was found that the table agreed with BESSEL's observed refractions to the distance of 88° from the zenith, which is as far as his determinations can be depended on, with such small discrepancies as may be supposed to exist in the observations themselves. So close an agreement between the theoretical and observed mean refractions was very unexpected, and even contrary to the opinion very generally held on this subject.

Astronomers are in the habit of using different tables or formulas of refraction, which, being derived from conjectural views, do not agree with one another, except to a limited distance from the zenith. Now this is contrary to the very conception we have of the mean refractions, which are determinate and invariable numbers, at least at the same observatory. A great advantage would therefore ensue from setting aside every uncertain table, and substituting in its place one deduced from the causes really existing in nature that produce the phenomena. Such a table adapted to every observatory, if this were found necessary, would contribute to the advancement of astronomy by rendering the observations made at different places more accurately comparable. It might contribute to the advancement of knowledge in another respect: for if the mean refractions were accurately settled, the uncertainty in the place of a star would fall upon the occasional corrections depending on the indications of the meteorological instruments; and it is not unreasonable to expect that much which is at present obscure and perplexing on this head might be cleared up, if it were separated from all foreign irregularities, and made the subject of the undivided attention of observers.

7. The paper in the Philosophical Transactions for 1823 takes into account only the rate at which the densities in a mean atmosphere vary at the surface of the earth; what follows is an attempt to complete the solution of the problem by estimating the effect of all the quantities on which the density at any height depends. For this purpose it will be requisite to employ certain functions of a particular kind, viz.

$$R_1 = 1 - c^{-u},$$

$$R_2 = 1 - u - c^{-u},$$

$$R_3 = 1 - u + \frac{u^2}{1.2} - c^{-u},$$

$$\cdot$$

$$\cdot$$

$$\cdot$$

$$R_i = \left(1 - u + \frac{u^2}{1.2} - \frac{u^3}{1.2.3} \cdots \pm \frac{u^{i-1}}{1.2.3 \ldots i-1}\right) - c^{-u}.$$

In these expressions c is the number of which the hyperbolic logarithm is unit; and it is obvious that R_i is zero when $u = 0$. These expressions have several remarkable properties, which are proved by merely performing the operations indicated.

1st. $$\frac{d.R_i}{du} = -R_{i-1},$$

$$\int -R_i\, du = R_{i+1},$$

the integral being taken equal to zero, when $u = 0$.

2ndly. $$\frac{d.c^{-u}R_i}{c^{-u}.du} = -(R_{i-1} + R_i),$$

$$\frac{d\,d\,.\,c^{-u}R_i}{c^{-u}\,.\,du^2} = R_{i-2} + 2R_{i-1} + R_i$$

$$\frac{d^3\,.\,c^{-u}R_i}{c^{-u}\,d\,u^3} = -(R_{i-3} + 3R_{i-2} + 3R_{i-1} + R_i),$$

&c.

3rdly. n being less than i,

$$\int \frac{d^n\,.\,c^{-u}R_i}{c^{-u}\,.\,du^n}\,du = (-1)^n \cdot \frac{d^n\,.\,c^{-u}R_{i+1}}{c^{-u}\,.\,du^n}.$$

These things being premised, the temperature of an atmosphere in equilibrium will have for its complete expression this formula,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fR_1 - f'\frac{d\,.\,c^{-u}R_2}{c^{-u}\,.\,du} - f''\cdot\frac{d^2\,.\,c^{-u}R_3}{c^{-u}\,.\,du^2} - \&c. \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

the coefficients f, f', f'', &c. being indeterminate constant quantities. A little attention will show that this expression is equivalent to a series of the powers of u; for, first, let the differential operations in the several terms be performed, which will bring out

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fR_1 + f'(R_2 + R_3) - f''(R_3 + 2R_4 + R_5) + \&c.;$$

next, expand R_1, R_2, &c., and the result will be,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fu + (f - f')\cdot\frac{u^2}{1.2}$$

$$- (f - 2f' + f'')\cdot\frac{u^3}{1.2.3}$$

$$+ (f - 2f' + 3f'' - f''')\frac{u^4}{1.2.3.4}$$

$$- \&c.$$

The intention of assuming the formula (7.) is to express the temperature in terms of such a form as will produce, in the refraction, independent parts that decrease rapidly.

In order to elucidate what is said, and more especially to prove that the analysis here followed comprehends all atmospheres, whether of dry air or of air mixed with aqueous vapour; let p', ϱ', τ' denote, as before, the pressure, the density, and the temperature, at the surface of the earth; and put p, ϱ, τ for the like quantities at the elevation z above the surface: the equations of equilibrium are these two, the radius of the earth being represented by a, viz.

$$p = \int \frac{-dz\,.\,\varrho}{\left(1 + \frac{z}{a}\right)^2}$$

$$\frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'}\cdot\frac{\varrho}{\varrho'}.$$

The second of these equations has already been noticed: the integral in the first being extended to the top of the atmosphere, is equal to the weight of the column of air above the initial height, every infinitesimal mass being urged by a gravitation which is equal to unit at the earth's surface, and decreases in the inverse proportion of the square of the distance from the earth's centre. By putting

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1-q, \quad \sigma = \frac{z}{1+\frac{z}{a}}, \quad \frac{\varrho}{\varrho'} = c^{-u},$$

the same two equations will be thus written, viz.

$$p = \varrho' \int - d\sigma\, c^{-u},$$

$$p = p'(1-q)\, c^{-u}.$$

The three quantities u, q, σ, are severally equal to zero at the earth's surface: and the two values of p will not be identical, unless the same three quantities can be expressed by functions of one variable, or, which is equivalent, unless two of them, as q and σ, are each functions of the remaining one u. Now q being a function of u, we shall have,

$$q = \frac{dq}{du}\cdot u + \frac{d\,dq}{du^2}\cdot\frac{u^2}{1.2} + \frac{d^3q}{du^3}\cdot\frac{u^3}{1.2.3} \text{ \&c.},$$

the differentials being valued when $u = 0$, that is, the particular values which they have at the earth's surface being taken. According to what was before shown, we have this other series for q, viz.

$$q = fu - (f-f')\cdot\frac{u^2}{1.2} + (f-2f'+f'')\cdot\frac{u^3}{1.2.3} \text{ \&c.:}$$

and as the two series must be identical, it follows that the quantities f, f', f'', &c., will be known, if we can ascertain the particular values assumed at the surface of the earth by the differentials of q considered as varying with u, or with the density. Thus the coefficients in the formula (7.) are not hypothetical quantities, but such as have a real existence in nature, and which might be determined experimentally, if we had the means of observing the phenomena of the atmosphere with sufficient exactness, so as to be able to determine q when u is given. It is further to be observed, that the same formula is general for all atmospheres, whether the air be entirely dry, or mixed with aqueous vapour: for it has been investigated from equations common to all atmospheres in equilibrium, without any consideration of a particular state of the air.

By substituting the series for q in the equation

$$\frac{p}{p'} = (1-q)\, c^{-u},$$

we obtain,

$$\frac{p}{p'} = c^{-u} - f c^{-u} R_1 - f' \frac{d\,.\,c^{-u} R_2}{du} - f''\,.\,\frac{d^2\,.\,c^{-u} R_3}{du^2} - \text{ \&c.} \quad \ldots\ldots \quad (8.)$$

Further, if this value of $\frac{p}{p'}$ be substituted in the equation

$$\frac{p}{p'} = \frac{q'}{p'} \int - d\sigma\, c^{-u},$$

we shall find

$$\int - d\sigma\, c^{-u} = \frac{p'}{q'} \cdot \left\{ c^{-u} - f \cdot c^{-u} R_1 - f' \frac{d \cdot c^{-u} R_2}{du} - \&c. \right\}.$$

Now, let this expression be differentiated; then divided by c^{-u}; and, finally, integrated, attending to the nature of the functions concerned; and the following result will be obtained:

$$\sigma = \frac{z}{1 + \frac{z}{a}} = \frac{p'}{q'} \left(u - f \cdot \frac{d \cdot c^{-u} R_2}{c^{-u} \cdot du} - f' \frac{d\, d \cdot c^{-u} R_4}{c^{-u} \cdot du_2} - \&c. \right) \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

The equations (7.), (8.), (9.) contain the theoretical explanation of the properties of the atmosphere. What is said may easily be proved by applying them to such phenomena as have been ascertained in a satisfactory manner. This application is besides necessary for determining the numerical values of the coefficients f, f', f'', &c., which enter into the expression of the refraction. For this purpose it is requisite to find the relations that subsist between the pressure, the temperature, and the height above the earth's surface, by combining the equations so as to exterminate u.

By performing the differentiations in the equation (9.), there will be obtained,

$$\sigma = \frac{p}{\rho} \{ u + f (R_1 + R_2) - f' (R_2 + 2 R_3 + R_4) + \&c.:$$

and, by expanding the functions,

$$\sigma = \frac{p'}{\rho'} \cdot \left\{ (1 + f) u - (2f - f') \cdot \frac{u^2}{2} + (2f - 3f' + f'') \frac{u^3}{1.2.3} - \&c. \right\}.$$

Now, by reverting the series for q, we get

$$u = \frac{q}{f} + \frac{f - f'}{2f} \cdot \frac{q^2}{f^2} + \frac{2f^2 - 4ff' + 3f'^2 - ff''}{6f^3} \cdot \frac{q^3}{f^3} + \&c.;$$

and, by substituting this value of u, the following formula will be obtained:

$$\sigma = \frac{z}{1 + \frac{z}{a}} = \frac{p'}{\rho'} \cdot \left(\frac{1 + f}{f} \cdot q + \frac{f - f' - f^2}{2f^3} \cdot q^2 + \&c. \right) \quad . \quad . \quad . \quad . \quad . \quad (A.)$$

This equation between the perpendicular elevation z, and the difference of temperature

$$q = \frac{\beta (\tau' - \tau)}{1 + \beta \tau'},$$

contains the law according to which the heat decreases as the height above the earth's surface increases.

Further, from the equation

$$\frac{p}{p'} = (1-q)\, c^{-u},$$

we deduce

$$\log\left(\frac{p'}{p}\right) = u + \log\frac{1}{1-q} = u + q + \frac{q^2}{2} + \frac{q^3}{3} + \&c.;$$

and, by substituting the value of q,

$$\log\frac{p'}{p} = (1+f)\,u - \frac{f - f' - f^2}{2}u^2 + \frac{f - 2f' + f'' - 3f^2 + 3ff' + 2f^3}{6}u^3, \&c.$$

By means of this series and the value of σ in terms of u already found, it is easy to deduce

$$\sigma = \log\left(\frac{p'}{p}\right) \times \frac{p'}{\varrho'}\left(1 - \frac{f}{2}\cdot u + \frac{2f - 2f' + 3f^2 - 3ff' - f^3}{12(1+f)}\cdot u^2 - \&c.\right);$$

and, by substituting the value of u, we finally obtain

$$\sigma = \frac{z}{1+\frac{z}{a}} = \log\frac{p'}{p} \times \frac{p'}{\varrho'}\cdot\left(1 - \frac{1}{2}q - \frac{f + f^2 - f'}{12f^2(1+f)}\cdot q^2, \&c.\right). \quad . \quad . \quad (B.)$$

This formula determines a perpendicular ascent z, when the difference of the pressures, and of the temperatures, at its upper and lower extremities, have been found.

The formulas that have been investigated are true in an atmosphere of air mixed with aqueous vapour, as well as in one of perfectly dry air; but in applying them, perspicuity requires that the two cases be separately considered.

Atmosphere of dry air.

In applying the formula (A.) to the experimental ascents that have been made in the atmosphere, σ may be accounted equal to z, the height ascended: for $\frac{z}{a}$, which is a minute fraction at the top of the atmosphere, is insensible in small elevations. Further, in such experiments, the depression of the thermometer, or the difference of the temperature at the upper and lower extremities of the ascent, is only a moderate number of degrees; and as β is a very small fraction, the value of q in the formula

$$q = \frac{\beta(\tau' - \tau)}{1 + \beta\tau'},$$

will be so inconsiderable, that its powers may be neglected. Attending to what is said, the formula (A.), even in those cases where the ascents are most considerable, may take this very simple form without much error, or rather with all the accuracy warranted by the nature of such experiments, viz.

$$z = \frac{p'}{\varrho'}\cdot\frac{1+f}{f}\cdot\frac{\beta(\tau' - \tau)}{1 + \beta\tau'};$$

or, by making $D = \varrho'(1 + \beta\tau')$,

$$z = \frac{1+f}{f}\cdot(\tau' - \tau)\,.\,\beta\cdot\frac{p'}{D}.$$

Now it is obvious that D is the density of the air at the earth's surface, reduced to zero of the thermometer; and hence we learn that $\frac{p'}{D}$ is independent on the magnitude of p', and has the same value in all atmospheres of dry air; for, D being the density of the air produced by the pressure p' at the fixed temperature zero of the thermometer, it will vary proportionally to p'.

The value of the constant quantity $\frac{p'}{D}$ is next to be found. It has been ascertained, by very careful experiments, that the density of mercury is to the density of dry air as 10462 to 1, the temperature being 0° centigrade, or 32° of FAHRENHEIT's scale, and the barometric pressure $0^{m}{\cdot}76$, or 29·9218 English inches. The temperature remaining at 32° FAHRENHEIT, if the barometric pressure be changed to p', the density of mercury will be to the density of dry air, at the temperature 32° FAHRENHEIT and under the pressure p', as $10462 \times \frac{29{\cdot}9218}{p'}$ to 1; wherefore, as D stands for the density of dry air in the circumstances mentioned, its value estimated in parts of the density of mercury, will be thus expressed:

$$D = \frac{1}{10462} \times \frac{p'}{29{\cdot}9218};$$

hence

$$\frac{p'}{D} = 10462 \times 29{\cdot}9218;$$

and, by reducing the inches to fathoms,

$$\frac{p'}{D} = L = 4347{\cdot}8 \text{ fath.}$$

This quantity being found, we deduce from the foregoing formula for z,

$$\frac{1+f}{f} = \frac{1}{\beta L} \times \frac{z}{\tau' - \tau}.$$

A single experiment in which z and $\tau' - \tau$ were ascertained, should be sufficient for determining $\frac{1+f}{f}$ and f: but it is well known that great irregularity prevails in the rate at which the heat decreases in the atmosphere, more especially when the elevations are small. This is owing chiefly to the thermometer, which is often affected by local and temporary causes. When we reflect that a considerable variation in the height is required to produce a small change of the thermometer, even the errors unavoidable in the use of that instrument must produce notable discrepancies in the rate, when the whole observed difference of temperature is only a few degrees. It thus appears that the quantity sought cannot be determined with tolerable exactness, except by taking a mean of the results obtained from many experiments. In this view, the average estimations of the decrease of heat in the atmosphere, which have been inferred from their own researches by philosophers on whose judgement and accuracy dependence can be had, becomes very valuable. Professor PLAYFAIR, in his

Outlines, states that the decrease of heat is nearly uniform for the greatest heights we can reach; and that it may be taken on an average as equal to 1° of FAHRENHEIT's thermometer for 270 feet, or 45 fathoms, of perpendicular ascent. The same rate has the authority of Professor LESLIE, to whom Meteorology is so much indebted. If we make $z = 45$ fathoms, $\tau' - \tau = 1°$, $\beta = \frac{1}{480}$, we shall obtain

$$\frac{1+f}{f} = \frac{45}{9{\cdot}05} = 5, f = \frac{1}{4},$$

which are the numbers assumed in the paper of 1823.

According to Dr. DALTON, another eminent philosopher who has studied meteorology very successfully, and made many experiments with great care, the average ascent for depressing FAHRENHEIT's thermometer 1° is 300 feet, or 50 fathoms: this gives

$$\frac{1+f}{f} = \frac{50}{9{\cdot}05} = 5{\cdot}5.$$

RAMOND, in his Treatise on the Barometrical Formula, has recorded the heights for depressing the centigrade thermometer 1°, in 42 different experiments. Setting aside four of this great number on account of their excessive irregularity, he states the mean of the remaining 38 at $164^{m}{\cdot}7$. A good average may be expected from so many experiments, made by observers of the greatest eminence, in different quarters of the world, in every variety of height and temperature. Now 4347·8 fathoms $= 7951^{m}$; $\beta = \frac{3}{800}$; $z = 164{\cdot}7$; consequently

$$\frac{1+f}{f} = \frac{800 \times 164{\cdot}7}{7951 \times 3} = 5{\cdot}5$$

It would be a great omission in this research to leave out the celebrated ascent of GAY LUSSAC in a balloon. According to LAPLACE, the whole height ascended, or z, is 6980^{m}, the depression of the thermometer, or $\tau' - \tau$, being 40°·25 centigrade: hence

$$\frac{1+f}{f} = \frac{800}{7951 \times 3} \times \frac{6980}{40{\cdot}25} = 5{\cdot}8.$$

It is to be observed that, although experience and theory both concur in proving that z and $\tau' - \tau$ increase together in the same proportion to considerable elevations in the atmosphere, yet, at very great elevations, there is no doubt that z increases in a greater ratio than $\tau' - \tau$: so that when very great heights are used for computing $\frac{1+f}{f}$, the resulting value will be greater than the true quantity. What is said accounts sufficiently for the excess of $\frac{1+f}{f}$ deduced from GAY LUSSAC's ascent, above the other values found from moderate elevations. Without further research we may adopt the following determinations as near approximations derived from a multitude of experiments,

$$\frac{1+f}{f} = 5{\cdot}5\,; f = \frac{2}{9}.$$

The difference of these numbers from those used in the paper of 1823, produces an increase in the refractions, amounting to 19″ at the horizon, and to 2″ at 2° of altitude.

The irregular manner in which the heat decreases in such experiments as have been used for finding f, evidently makes them altogether unfit for determining the next coefficient f'. One remark respecting this quantity deserves to be noticed. By expanding the formula (4.), we obtain,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fu + f\cdot\frac{u^2}{1.2} - f\cdot\frac{u^3}{1.2.3} + \&c.:$$

the exact value is

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fu + (f-f')\cdot\frac{u^2}{1.2} - \&c.:$$

now, as these values continue very nearly equal to considerable elevations, the first terms of the two series must nearly coincide: which requires that f' shall be only a small part of f.

We have next to attend to the formula (B.). As q is only a small fraction in all the elevations that have been reached in the atmosphere, its square and other powers may be neglected: so that,

$$\sigma = \frac{z}{1+\frac{z}{a}} = \frac{p'}{p'}\left(1-\frac{q}{2}\right)\log\left(\frac{p'}{p}\right):$$

and, because

$$1-\frac{q}{2} = \frac{1+\beta\left(\frac{\tau'+\tau}{2}\right)}{1+\beta\tau'}\,;\quad \frac{p'}{p'(1+\beta\tau')} = \mathrm{L}:$$

$$z = \left(1+\frac{z}{a}\right).\mathrm{L}\left(1+\beta\left(\frac{\tau'+\tau}{2}\right)\right).\log\left(\frac{p'}{p}\right).$$

Now this is nothing more than the usual barometric formula for measuring heights, as it is found in the writings of Laplace or Poisson. It supposes that unit represents the force of gravity at the earth's surface; and if the variable intensity of that force in different latitudes must be taken into account, nothing more is requisite than to multiply by the proper factor. When this is done the foregoing expression will be identical with the usual formula, all its minutest corrections included. But there is this difference between the two cases, that the usual formula is investigated on the arbitrary supposition that the temperature is constant at all the points of an elevation, and equal to the mean of the temperatures at the two extremities; whereas the other expression is strictly deduced from the general properties of an atmosphere in equilibrium. The exact theoretical formula has been made to coincide with the approximate one, by dismissing all the terms that cannot be estimated in the present state of our knowledge of the phenomena of the atmosphere.

All the properties of the atmosphere that have been ascertained with any degree of certainty, have been made known to us by the application of the barometric formula:

it would therefore be superfluous to attempt, by the consideration of particular experiments, any further elucidation of a theory which is, in a manner, identical with observation, as far as our knowledge extends.

Atmosphere of air mixed with aqueous vapour.

Continuing to represent the pressure and temperature at the earth's surface by p' and τ', and the like quantities at the height z by p and τ, the symbols (ϱ'), (ϱ) may be used to denote the respective densities in the case of air mixed with aqueous vapour. When the pressure and density vary, all the gases, and mixtures of gases and vapours, are found to follow the same laws of dilatation and compression: and hence the same equations that express the equilibrium of an atmosphere of dry air, will hold equally in one of moist air. In the present case these equations will therefore be,

$$p = \int \frac{-\,dz\,.\,(\varrho)}{\left(1+\frac{z}{a}\right)^2},$$

$$\frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'}\cdot\frac{(\varrho)}{(\varrho')}:$$

and if we put

$$\sigma = \frac{z}{1+\frac{z}{a}}, \quad \frac{1+\beta\tau}{1+\beta\tau'} = 1-q, \quad \frac{(\varrho)}{(\varrho')} = c^{-u},$$

the same equations will be thus written,

$$p = (\varrho')\int -\,d\sigma\, c^{-u},$$

$$p = p'\,(1-q)\,c^{-u}.$$

The three quantities σ, q, u are severally equal to zero at the surface of the earth: so that, by the same procedure as before, we shall obtain these formulas,

$$q = fu - (f-f')\frac{u^2}{2} + \&c.$$

$$\sigma = \frac{p'}{(\varrho')}\cdot\left\{u - f\,.\,\frac{d\,.\,c^{-u}\,R_2}{c^{-u}\,du} - f'\,.\,\frac{dd\,.\,c^{-u}\,R_2}{c^{-u}\,du^2} - \&c.\right.$$

But it is to be observed that, in these expressions, the coefficients f, f', &c., are not exactly the same as in an atmosphere of dry air: for the quantities mentioned, although they have determinate values in the same quiescent atmosphere, depend upon the manner in which the temperature q, or the height z, varies relatively to the density, or to u.

If we suppose that the height z is not very great, so that the powers of q may be

* This equation is equivalent to the one in p. 18 of M. Biot's dissertation, on which that author lays so much stress.

neglected, we shall obtain from the foregoing equations,

$$z = \frac{p'}{(\varrho')} \cdot \frac{1+f}{f} \cdot q :$$

and hence

$$\frac{1+f}{f} = \frac{1+\beta\tau'}{\beta} \cdot \frac{(\varrho')}{p'} \cdot \frac{z}{\tau' - \tau}.$$

In order to ascertain how far this value is different from the like value in the case of dry air, we must resolve the complex density (ϱ') into its elements. The hygrometer will discover the tension of the vapour at the earth's surface; and if ϕ' denote this tension in inches of mercury, and ϱ' be the density of dry air under the pressure p' and at the temperature τ', the following equation is proved in all the late treatises on Natural Philosophy,

$$(\varrho') = \varrho' \left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right) :$$

by means of which we obtain

$$\frac{1+f}{f} = \frac{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}}{\beta \mathrm{L}} \times \frac{z}{\tau' - \tau},$$

$$\mathrm{L} = \frac{p'}{\varrho' (1 + \beta\tau')}.$$

Now the small additional factor in the value of $\frac{1+f}{f}$ is not taken into account in the measurement of heights by the barometer, no distinction being usually made between dry air and moist air. In order to form some estimate of its effect, we may instance the mean atmosphere of our climate, the temperature of which is 50° Fahrenheit; the greatest possible tension of vapour in such an atmosphere is ·36 of an inch of mercury; at a medium, if we make $\phi' = \cdot 18$, and $p' = 30$ inches, we shall have,

$$1 - \frac{3}{8} \cdot \frac{\phi'}{p'} = 1 - \frac{1}{444}.$$

It thus appears that in our climate, when the mean portion of aqueous vapour is mixed with the air, the value of $\frac{1+f}{f}$ is less than it would be if the air were perfectly dry by its $\frac{1}{444}$th part, a quantity too minute to be perceptible in most experiments. A small part only of the refractions depend upon f, about a twelfth part of the whole at the horizon; so that, neglecting the minute variations which f undergoes by the greater or less portions of aqueous vapour mixed with the air, the effect of which on the refractions is insensible, we may assume that it has the same value in all atmospheres. The same thing applies with greater force to the other coefficients f', f'', &c., which having themselves hardly any influence on the refractions, their minute changes in different atmospheres may be wholly disregarded.

If we substitute for (ϱ') its equivalent $\varrho' \left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right)$ in the foregoing value of σ,

we shall obtain the following equation, which is sufficient for the problem of the refractions in an atmosphere of moist air:

$$\sigma = \frac{1}{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}} \cdot \frac{p'}{\rho'} \cdot \left\{ u - f \cdot \frac{d \cdot c^{-u} R_2}{c^{-u} du} - f' \cdot \frac{dd \cdot c^{-u} R_3}{c^{-u} du^2} - \&c. \right\} \quad . \quad . \quad . \quad (10.)$$

In which expression the coefficients f, f', &c., may be considered the same in all atmospheres, the quantity u varying from zero at the earth's surface to be infinitely great at the top of the atmosphere.

8. In the foregoing analysis, every formula has been strictly deduced from the equations of equilibrium: no quantities have been introduced except such as really exist in nature, and might be determined experimentally, if we had the means of exploring the phenomena of the atmosphere with the requisite accuracy. It may not be improper to notice here an obvious consequence of the equation

$$p = \xi' \int - d\sigma\, c^{-u},$$

which holds in an atmosphere of dry air; namely, that the integral

$$\int - d\sigma\, c^{-u},$$

being extended from the surface of the earth to the top of the atmosphere, is the analytical expression of $\frac{p'}{\xi'}$, or of the height of the homogeneous atmosphere, that is, of a column of air equiponderant to the whole atmosphere, and every part of which has the same density and the same weight which it would have at the surface of the earth. This height varies only with the temperature, and is thus determined:

$$\frac{p'}{\rho'} = \frac{p'}{\rho' (1 + \beta \tau')} \cdot (1 + \beta \tau') = \frac{p'}{D} (1 + \beta \tau') = L (1 + \beta \tau').$$

In like manner, in an atmosphere of air mixed with aqueous vapour, the same integral is equal to $\frac{p'}{(\rho')}$: and we have

$$\frac{p'}{(\rho')} = \frac{p'}{\rho'} \cdot \frac{1}{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}} = \frac{L (1 + \beta \tau')}{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}}.$$

Thus the analytical theory agrees in every respect with the real properties of the atmosphere, as far as these have been ascertained; and we now proceed to show that the same theory represents the astronomical refractions with a fidelity that can be deemed imperfect only in so far as the constants f, f', &c., which can only be determined by experiment, are liable to the charge of inaccuracy.

9. The apparent zenith-distance of a star being represented by θ, and the refraction by $\delta\theta$, the following formulas have already been obtained (§ 2. equations (2.) and (3.)).

$$d \cdot \delta\theta = \frac{dy}{\sqrt{r^2 - y^2}},$$

$$y = a \sin \theta \times \sqrt{\frac{1 + 2\,\phi(\rho')}{1 + 2\,\phi(\rho)}},$$

the quantity $\delta\theta$ being supposed to increase from the surface of the earth to the top of the atmosphere. For the sake of perspicuity, we shall, in the first place, confine our attention to an atmosphere of dry air, in which case it is known by experiment that the refractive power $\phi(\varrho)$ is proportional to the density ϱ; so that

$$\phi(\varrho) = \mathrm{K} \times \varrho,$$

K being a constant. Adverting to the mode of expression before used, we have

$$\varrho = \varrho' c^{-u};$$

and hence

$$\phi(\varrho) = \mathrm{K} \times \varrho = \mathrm{K}\varrho' \cdot c^{-u},$$

$$y = a \sin \theta \times \sqrt{\frac{1 + 2\,\mathrm{K}\rho'}{1 + 2\,\mathrm{K}\rho' c^{-u}}};$$

and by introducing new symbols in order to abridge expressions,

$$\alpha = \frac{\mathrm{K}\rho'}{1 + 2\,\mathrm{K}\rho'},$$

$$\omega = 1 - c^{-u},$$

$$y = \frac{a \sin \theta}{\sqrt{1 - 2\,\alpha\,\omega}}.$$

Let this value of y be substituted in the differential of the refraction; then

$$r^2 = (a + z)^2 = a^2 \left(1 + \frac{z}{a}\right)^2 = \frac{a^2}{\left(1 - \frac{\sigma}{a}\right)^2},$$

$$d \cdot \delta\theta = \sin\theta \times \frac{\alpha}{1 - 2\,\alpha\,\omega} \times \frac{d\omega}{\sqrt{\frac{1 - 2\,\alpha\,\omega}{\left(1 - \frac{\sigma}{a}\right)^2} - \sin^2\theta}}.$$

In further transforming this expression, it is to be observed that α is a very small fraction less than ·0003; and if the atmosphere extend fifty miles above the earth's surface, $\frac{z}{a}$ or $\frac{\sigma}{a}$ when greatest will not exceed ·012. If we now put

$$\frac{\sigma}{a} = \frac{s}{a} + \alpha\,\omega,$$

we shall have

$$\frac{1 - 2\,\alpha\,\omega}{\left(1 - \frac{\sigma}{a}\right)^2} = \frac{(1 - \alpha\,\omega)^2 - \alpha^2\omega^2}{\left(1 - \alpha\,\omega - \frac{s}{a}\right)^2} = 1 + 2\,\frac{s}{a} + 3\,\frac{s^2}{a^2},$$

the quantities rejected being plainly of no account relatively to those retained. Further, because ω is always less than 1, $\frac{\alpha}{1 - 2\,\alpha\,\omega}$ is contained between α and $\alpha(1 + 2\,\alpha)$; and

it may be taken equal to α, or to the mean value $\alpha\,(1+\alpha)$. Thus we have

$$d.\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)\,du\,c^{-u}}{\sqrt{\cos^2\theta + 2\frac{s}{a} + 3\frac{s^2}{a^2}}}.$$

Again, the formula (9.) gives

$$\sigma = s + a.\alpha\omega = \frac{p'}{\rho'}\cdot\left\{u - f'.\frac{d.c^{-u}R_2}{c^{-u}du} - f'.\frac{dd.c^{-u}R_4}{c^{-u}.du^2} - \&c.\right\}.$$

Now,

$$\frac{p'}{\rho'} = \frac{p'}{\rho'(1+\beta\tau')}\cdot(1+\beta\tau') = \mathrm{L}(1+\beta\tau'):$$

and if we make

$$s.\frac{g'}{p'} = \frac{s}{\mathrm{L}(1+\beta\tau')} = x,$$

$$\frac{p'}{\rho'}\cdot\frac{1}{a} = \frac{\mathrm{L}(1+\beta\tau')}{a} = i,$$

$$\frac{a.\rho'.\alpha}{p'} = \frac{a}{i} = \lambda,$$

we shall have

$$\frac{s}{a} = ix$$

$$x = u - \lambda(1-c^{-u}) - f'.\frac{d.c^{-u}R_2}{c^{-u}du} - f'\frac{dd.c^{-u}R_4}{c^{-u}du^2} - \&c.$$

Let $\Psi(u)$ stand for all the terms in this value of x except the first, so that

$$x = u - \Psi(u):$$

from this we deduce by LAGRANGE's theorem,

$$c^{-u} = c^{-x} - c^{-x}\Psi(x) - \frac{1}{2}\cdot\frac{d.c^{-x}\Psi^2(x)}{dx} - \&c.:$$

consequently,

$$du\,c^{-u} = dx\,c^{-x} + \frac{d.c^{-x}\Psi(x)}{dx}dx + \frac{1}{2}\cdot\frac{dd.c^{-x}\Psi^2(x)}{dx^2}dx + \&c.$$

By means of the values that have been found, the differential of the refraction can be expressed in terms of one variable x. In making the substitutions, the smallest term of the radical quantity is to be neglected in all the terms of $du\,c^{-u}$, except the first and greatest; and the denominator of that term is to be expanded. Thus we obtain

$$d.\delta\theta = \sin\theta.\alpha(1+\alpha).\left\{\int\frac{dx}{\sqrt{\cos^2\theta + 2ix}}\cdot\left(c^{-x} + \frac{d.c^{-x}\Psi(x)}{dx}\right)\right.$$

$$+\frac{1}{2}\int\frac{dx}{\sqrt{\cos^2\theta+2ix}}\cdot\frac{dd.c^{-x}\Psi^2(x)}{dx}$$

$$\left.-\frac{3}{2}\int\frac{dx.c^{-x}.i^2x^2}{(\cos^2\theta+2ix)^{\frac{5}{2}}}\right.$$

In order to estimate the relative magnitude of the several parts of this formula we

must find the numerical values of the quantities α and i. If η stands for the refraction at 45° of altitude, determined very exactly from many astronomical observations, we shall have

$$\alpha = \eta (1 - 2i + 2\eta),$$

as will readily appear from the formula according to Cassini's method given in § 1. MM. Biot and Arago have ascertained the value of α with great exactness in a different way, by means of experiments on the gases with the prism. In some of the best attempts to determine α, the refractions at 45° of altitude, being reduced to the barometer 29·6 and to the temperature 50° Fahr., are as follows:

Dr. Brinkley	57″·42
De Lambre	57·58
Bessel, Tab. Reg	57·55
Experiments of MM. Biot and Arago	57·65
Mean	57·55

It appears that Bessel's determination has the best claim to be preferred: but as it differs very little from De Lambre's result, which is adopted in the paper of 1823, the same value will be retained in the calculations which follow. According to De Lambre, the value of α is 60″·616* at the temperature 0° centigrade, and the barometric pressure 0^m·76: wherefore, when the temperature is 50° Fahr. and the pressure 30 inches (= 0^m·762), we shall have

$$\alpha = 60{\cdot}616 \times \frac{762}{760 \times 1{\cdot}0018} \times \frac{1}{1 + \frac{18}{480}} = 58''{\cdot}47:$$

and in parts of the radius,

$$\alpha = {\cdot}0002835.$$

It has been found that L = 4347·8 fathoms at 0° centigrade or 32° Fahr.: wherefore, if we make a = mean radius of the earth = 3481280 fathoms, we shall have at the temperature of our climate, or 50° Fahr.,

$$i = \frac{L(1 + \beta\tau')}{a} = \frac{4347{\cdot}8\left(1 + \frac{18}{483}\right)}{3481280} = {\cdot}0012958;$$

and hence

$$\lambda = \frac{\alpha}{i} = {\cdot}21878.$$

We can now inquire into the values of the last two terms of the foregoing formula for the refraction, both of which are very small. With respect to the first of them, we have,

$$\Psi(x) = \lambda(1 - c^{-x}) + f' \cdot \frac{d \cdot c^{-x} R_2}{c^{-x} dx} + f'' \cdot \frac{d\,d \cdot c^{-x} R_4}{c^{-x} dx^2} - \&c.:$$

* Tableaux Chronomiques, publiées par le Bureau des Longitudes de France.

and, by performing the differential operations,

$$\Psi(x) = \lambda(1 - c^{-x}) + f(R_1 + R_2) + f'(R_2 + 3R_3 + R_4);$$

and, by substituting the values of the functions,

$$h = 2f - \lambda = \cdot 22566$$

$$\Psi(x) = -h(1 - c^{-x}) + f'x + 4f'\left(1 - x + \frac{3x^2}{8} - \frac{x^3}{24} - c^{-x}\right).$$

It might not be very objectionable to neglect the term multiplied by f', for the same reasons that the terms which follow it are neglected, that is, both on account of the nature of the functions and because the coefficients are small: but, in order to leave no room for scruples respecting accuracy, the square of the entire expression set down, may be thus represented:

$$\Psi^2(x) = G - 8hf' \cdot G' + 8ff' \cdot G'' + 16f'^2 \cdot G'''.$$

The integral in the term under consideration is greatest when the radical quantity in the denominator is least, that is, when $\cos\theta = 0$: and if the integration be performed betwen the limits $x = 0$, $x = \infty$, we shall obtain a result greater than if the integral were extended only to the top of the atmosphere. Now we have,

$$G = h^2(1 - 2c^{-x} + c^{-2x}) + 2hf \cdot xc^{-x} - 2hf \cdot x + f^2 \cdot x^2;$$

and, by operating on the terms separately, the part of the integral depending on G, will be as follows:

$$\int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x} G}{dx^2} =$$

$$\frac{\sqrt{\pi}}{\sqrt{2i}} \times \left(h^2(1 - 4\sqrt{2} + 3\sqrt{3}) - 3hf(\sqrt{2} - 1) + \frac{3}{4}f^2\right) = \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 00216.$$

The other parts depending on G', G'', G''' are complicated; but they are troublesome more on account of the number of terms they contain than from any difficulty in the integrations. The following results have been obtained:

$$8hf' \times \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x} G'}{dx^2} = -f' \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 01759,$$

$$8ff' \times \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x} G''}{dx^2} = -f' \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 02043,$$

$$16f'^2 \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x} G'''}{dx^2} = +f'^2 \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 00855.$$

Collecting all the parts, the term sought is found, viz.

$$\frac{\alpha(+\alpha)}{2} \cdot \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x} \Psi^2(x)}{dx^2} =$$

$$\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \times \left(\cdot 00108 - f' \times \cdot 00142 + f'^2 \times \cdot 00427\right).$$

To this must be added the other term, which, being integrated in the same circumstances, gives,

$$-\frac{3}{2}\cdot\int_0^\infty \frac{dx\,c^{-x}}{\sqrt{2ix}}\times\frac{ix}{2}=-\frac{3}{8}\cdot\frac{\sqrt{\pi}}{\sqrt{2i}}=-\frac{\sqrt{\pi}}{\sqrt{2i}}\times\cdot00049.$$

It thus appears that the two small terms of the expression of the refraction are, together, equal to

$$\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}}\cdot\left(\cdot00059-f'\times\cdot00142+f''^2\times\cdot00427\right):$$

and as

$$\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}}=2036''\cdot5,$$

the greatest amount of both is about $1''$.

The whole refraction will therefore be thus expressed:

$$d.\delta\theta=\sin\theta\times\alpha(1+\alpha)\int\frac{dx}{\sqrt{\cos^2\theta+2ix}}\cdot\left(c^{-x}+\frac{d.c^{-x}\Psi(x)}{dx}\right),$$

with the assurance that the error cannot exceed $1''$. If we substitute what $\Psi(x)$ stands for, we shall have

$$d.\delta\theta=\sin\theta\times\alpha(1+\alpha)\times\int\frac{dx}{\sqrt{\cos^2\theta+2ix}}\times$$

$$\left(c^{-x}+\lambda\cdot\frac{d.(c^{-x}-c^{-2x})}{dx}+f\cdot\frac{dd.c^{-x}R_2}{dx^2}+f'\cdot\frac{d^3.c^{-x}R_3}{dx^3}+\&c.\right).$$

This expression being regular, it may be continued to any number of terms, and it has the advantage of being linear with respect to the coefficients. Adverting to what x stands for, it will appear that $L\times x$ is nearly equal to s, or to z, that is, to the elevation in the atmosphere; so that, if we suppose the greatest height of the atmosphere is $10\times L$, or about fifty miles, the greatest value of x will be 10; and all the integrals in the foregoing expression must be taken between the limits zero and 10. But the quantity c^{-x} is so small when x has increased to 8 or 10, that the results are not sensibly different whether the integrals be extended to those limits or be continued to infinity. By substituting the values of the functions, the expression of $\delta\theta$ will take this form:

$$\delta\theta=\sin\theta\times\alpha(1+\alpha)\times\left\{\int\frac{dx\,c^{-x}}{\sqrt{\cos^2\theta+2ix}}+\lambda\int\frac{dx\,(2c^{-2x}-c^{-x})}{\sqrt{\cos^2\theta+2ix}}\right.$$

$$-f\int\frac{dx}{\sqrt{\cos^2\theta+2ix}}\cdot\left(4c^{-2x}-3c^{-x}+xc^{-x}\right)$$

$$+f'\int\frac{dx}{\sqrt{\cos^2\theta+2ix}}\cdot\left(8c^{-2x}-8c^{-x}+7xc^{-x}-2x^2c^{-x}+\frac{x^3c^{-x}}{6}\right)$$

$$-f''\int\frac{dx}{\sqrt{\cos^2\theta+2ix}}\cdot\left(16c^{-2x}-16c^{-x}+16xc^{-x}-\frac{15}{2}x^2c^{-x}\right.$$

$$\left.\left.+\frac{11}{6}xc^{-x}-\frac{5}{24}x^4c^{-x}+\frac{x^5c^{-x}}{120}\right).\right.$$

In order to illustrate the rapidity with which the terms decrease, it may be proper to find the limit of $\delta\theta$, by making $\cos^2\theta = 0$, and integrating between the limits $x = 0$, $x = \infty$; which limit is not sensibly different from the refraction at the horizon. Now it will be found that, in the circumstances mentioned,

$$\delta\theta = \frac{a(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \times \left\{ 1 + \lambda\left(\sqrt{2} - 1\right) - f\left(2\sqrt{2} - \frac{5}{2}\right) + f'\left(4\sqrt{2} - \frac{91}{16}\right) - f''\left(8\sqrt{2} - \frac{2895}{256}\right) - \&c. :\right.$$

or, in seconds,

$$\delta\theta = 2072''{\cdot}46 - f' \times 62{\cdot}4 - f'' \times 10''{\cdot}2 - \&c.$$

From this calculation it appears that the term multiplied by f'' and all the subsequent terms are too small to be sensible; and as f' is much less than f, even the term multiplied by f' can hardly exceed a few seconds at low altitudes. There is great probability that the horizontal refraction is very near $34'\ 30''$, and does not exceed this quantity.

To prepare the foregoing expression of $\delta\theta$ for integration, put

$$m = 10,\ \frac{\sqrt{2im}}{\cos\theta} = \tan\varphi,\ \ e = \tan\frac{\varphi}{2};$$

then

$$\cos^2\theta = \frac{(1-e^2)^2}{4e^2} \times 2im,$$

$$\sqrt{\cos^2\theta + 2im} = \frac{\sqrt{5i}}{e} \cdot \sqrt{(1-e^2)^2 + 4e^2 \cdot \frac{x}{m}} = \frac{\sqrt{5i}}{e} \cdot \Delta :$$

and we shall have

$$\left.\begin{aligned} \delta\theta = \sin\theta \times \frac{a(1+\alpha)}{\sqrt{5i}} \times \Big\{ & \int_0^m \frac{e\,dx}{\Delta} \cdot c^{-x} \\ & + \lambda \int_0^m \frac{e\,dx}{\Delta} \cdot \left(2c^{-2x} - c^{-x}\right) \\ & - f \int_0^m \frac{e\,dx}{\Delta} \cdot \left(4c^{-2x} - 3c^{-x} + x c^{-x}\right) \\ & + f' \int_0^m \frac{e\,dx}{\Delta} \cdot \left(8c^{-2x} - 8c^{-x} + 7x c^{-x} - 2x^2 c^{-x} + \frac{x^3}{6} c^{-x}\right). \end{aligned}\right\} \quad \text{(C.)}$$

For the sake of abridging, the several integrals in succession may be represented by Q_0, Q_1, Q_2, Q_3; so that the value of $\delta\theta$ will be thus written:

$$\delta\theta \sin\theta \times \frac{a(1+\alpha)}{\sqrt{5i}} \cdot (Q_0 + \lambda Q_1 - f Q_2 + f' Q_3).$$

10. The equation (C.) supposes that the atmosphere consists entirely of dry air: we have next to consider what modification must be made when it contains a portion of aqueous vapour.

In the first place, when p' and τ', the pressure and temperature at the surface of the earth, are given, as they are in the mean atmosphere which produces the refractions, the quantity α, or the refractive power of the air, is not liable to be altered by any possible mixture of aqueous vapour. For if an addition of vapour to dry air diminish the refractive power by making the density less, the greater action of the vapour upon light is found almost exactly to compensate the defect. LAPLACE first made this observation; which has been confirmed by MM. BIOT and ARAGO, who have established by experiments, that the refractive power of air, whether dry or mixed with vapour, is the same, when the pressure and temperature are the same. It thus appears that, as far as the quantity α, or the refractive power of the air at the earth's surface, is concerned, the astronomical refractions are independent on the hygrometric condition of the atmosphere.

But a mixture of vapour may produce changes in the expression of the refraction, by altering the coefficients or the integrals. Now, if we attend to the formulas that have been found for an atmosphere of moist air, and in the equation (10.) make the same substitution as in the case of dry air, viz.

$$\frac{\sigma}{a} = \frac{s}{a} + \alpha\,\omega,$$

we shall obtain

$$s \cdot \frac{(\rho')}{p'} = \frac{s\left(1 - \frac{3}{8}\,\frac{\phi'}{p'}\right)}{\mathrm{L}(1 + \beta\tau')} = x,$$

$$\frac{p'}{(\rho')} \cdot \frac{1}{a} = \frac{\mathrm{L}(1 + \beta\tau')}{a\left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right)} = i,$$

$$\alpha \cdot \frac{a(\rho')}{p'} = \frac{\alpha}{i} = \frac{a\left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right)}{\mathrm{L}(1 + \beta\tau')} = \lambda:$$

and further, it will appear that the same relation subsists between x and u in the atmosphere of moist air as between the quantities represented by the same letters in the atmosphere of dry air. The same procedure will therefore lead, in both cases, to the same integrals extending between the same limits. The only difference lies in the values of λ and i, which in the case of moist air acquire, as a multiplier or divisor, the small factor $\left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right)$ depending on the tension of the vapour at the earth's surface. If the hygrometer afforded an easy practical method of ascertaining the tension of the vapour, the minute variations of the refractions, arising from moisture in the atmosphere, might be corrected by the method usually employed for compen-

sating the small changes which a difference of temperature causes in the mean constants.

Experience confirms what has been said; for all the astronomers who have attended to aqueous vapour in the atmosphere, agree in admitting that it either has no influence, or but a very small and imperceptible effect, to alter the refractions. On this head it will be sufficient to cite the authority of M. Biot*, who seems carefully to have studied this point, on which he expresses himself very strongly. The very exact coincidence of the theoretical with the observed refractions as far as 88° or $88\frac{1}{2}°$ from the zenith, concurs to prove that the variable quantity of vapour in the air has little influence so long as it retains the gaseous form; but at lower altitudes, when the rays of light become almost parallel to the horizon, it is very probable that particular and local causes may come into play.

11. Nothing is now wanted for completing the solution of the problem, except the reducing of the expression (C.) to a form fit for numerical calculation.

Investigation of the integral $\mathbf{Q}_y$.

We have

$$Q_0 = \int_0^m \frac{e\,dx\,c^{-z}}{\Delta} = \int_0^m \frac{e\,dx\,c^{-z}}{\sqrt{(1-e^2)^2 + 4e^2 \cdot \frac{x}{m}}};$$

assume,

$$\Delta = \sqrt{(1-e^2)^2 + 4e^2 \cdot \frac{x}{m}} = 1 - e^2 + 2e^2 z;$$

then,

$$\frac{e\,dx}{\Delta} = e \,.\, m\,dz,$$

$$\frac{x}{m} = t = 1 - e^2(z - z^2).$$

By Lagrange's theorem,

$$\Psi = t - t^2,$$

$$z = t + e^2 \,.\, \Psi + \frac{e^4}{1.2} \cdot \frac{d.\Psi^2}{dt} + \frac{e^6}{1.2.3} \cdot \frac{d\,d.\Psi^3}{dt^2} + \&c.;$$

$$m\,dz = m\,dt\,. \left\{ 1 + e^2 \cdot \frac{d.\Psi}{dt} + \frac{e^4}{1.2} \cdot \frac{d^2\Psi^2}{dt^2} + \frac{e^6}{1.2.3} \cdot \frac{d^3\Psi^3}{dt^3} + \&c. \right\};$$

consequently,

$$\int_0^m \frac{e\,dx\,c^{-z}}{\Delta} = \int_0^1 m\,dt\,c^{-mt} \left\{ e + e^3 \frac{d\Psi}{dt} + \frac{e^5}{1.2} \cdot \frac{d^2\Psi^2}{dt^2} + \&c. \right\}.$$

Wherefore, if we assume

$$Q_0 = A_1 e + A_3 e^3 + A_5 e^5 + \&c.,$$

* Précis Elem. de Physique, p. 229. tom. ii. edit. 2nd. Addit. à la Conn. des Temps, 1839, p. 36.

we shall have

$$A_{2n+1} = \frac{1}{1.2.3\ldots n}\int_0^1 m\,dt\,c^{-mt}\cdot\frac{d^n.\Psi^n}{dt^n}.$$

In the first place, it may be proper to show that all the coefficients in the series for Q^0 are positive. For this purpose integrate by parts, and the results will be

$$A_{2n+1} = \frac{m}{1.2.3\ldots n}\times\left\{c^{-mt}\cdot\frac{d^{n-1}\Psi^n}{dt^{n-1}} + \int m\,dt\,c^{-mt}\cdot\frac{d^{n-1}\Psi^n}{dt^{n-1}}\right\}.$$

Now it is evident that

$$\frac{d^{n-1}\Psi^n}{dt^{n-1}}$$

is divisible both by t and $1-t$: it is therefore zero at both the limits of the integral; so that we have simply

$$A_{2n+1} = \frac{m}{1.2.3\ldots n}\cdot\int_0^1 m\,dt\,c^{-mt}\cdot\frac{d^{n-1}\Psi^n}{dt^{n-1}}.$$

Continuing to integrate in like manner, we shall find after n successive operations,

$$A_{2n+1} = \frac{m^n}{1.2.3\ldots n}\int_0^1 m\,dt\,c^{-mt}\Psi^n,$$

which is obviously a positive quantity.

By expanding, we get

$$\Psi^n = t^n(1-t)^n = t^n - n.t^{n+1} + n\cdot\frac{n-1}{2}\cdot t^{n+2} - \&c.:$$

and, by performing the differential operations,

$$\frac{1}{1.2.3\ldots n}\cdot\frac{d^n\Psi^n}{dt^n} = 1 - n.n+1\cdot\frac{t}{1} + \left(n\cdot\frac{n-1}{2}\right)\cdot n+1.n+2\cdot\frac{t^2}{1.2} - \&c.$$

Now, because $t = \frac{x}{m}$, if we put,

$$\Psi'(x) = \frac{1}{1.2.3\ldots n}\cdot\frac{d^n\Psi^n}{dt^n},$$

we shall have

$$\Psi'(x) = 1 - n\cdot\frac{n+1}{m}\cdot\frac{x}{1} + n\cdot\frac{n-1}{2}\cdot\frac{n+1.n+2}{m^2}\cdot\frac{x^2}{1.2} - \&c.$$

Another form may be given to this function; for, without any variation in quantity, t and $1-t$ may be interchanged, not only in

$$\Psi^n = t^n(1-t)^n,$$

but in all its differentials, observing that the results equal in quantity will have opposite signs when the number of differentiations is odd, and the same sign when the number is even. Now if, instead of $t = \frac{x}{m}$, we substitute $1-t = \frac{m-x}{m}$, we shall have

$$\Psi'(x) = \pm\left\{1 - n\cdot\frac{n+1}{m}\cdot\frac{m-x}{1} + n\cdot\frac{n-1}{2}\cdot\frac{n+1.n+2}{m^2}\cdot\frac{(m-x)^2}{1.2} - \&c.\right.$$

The coefficient A_{2n+1} is thus expressed in terms of x:

$$A_{2n+1} = \int_0^m dx\, c^{-x} \Psi'(x);$$

the indefinite integral is

$$-c^{-x} \cdot \left\{ \Psi'(x) + \frac{d \cdot \Psi'(x)}{dx} + \frac{d\,d \cdot \Psi'(x)}{dx^2} + \&c. \right\}.$$

This integral, taken between the limits $x = 0$ and $x = m$, is equal to A_{2n+1}: the first form of $\Psi'(x)$ will give the values of all the differentials at the limit $x = 0$; and the second form of the same function will give the like values at the other limit $x = m$: Thus we obtain,

$$A_{2n+1} \left\{ 1 - n \cdot \frac{n+1}{m} + n \cdot \frac{n-1}{2} \cdot \frac{n+1 \,.\, n+2}{m^2} - \&c. \right\}$$

$$\mp c^{-m} \cdot \left\{ 1 + n \cdot \frac{n+1}{m} + n \cdot \frac{n-1}{2} \cdot \frac{n+1 \,.\, n+2}{m^2} + \&c. \right\},$$

the upper or lower sign taking place according as n is even or odd.

The numerical coefficients, computed by the formula, are as follows:

$$c^{-m} = c^{-10} = 0000454$$

$$A_1 = 1 - c^{-m} = 0{\cdot}9999546$$

$$A_3 = \frac{4}{5} + \frac{6}{5} c^{-m} = 0{\cdot}8000545$$

$$A_5 = \frac{13}{25} - \frac{48}{25} c^{-m} = 0{\cdot}5199219$$

$$A_7 = \frac{7}{25} + \frac{72}{25} c^{-m} = 0{\cdot}2801326$$

$$A_9 = \frac{16}{125} - \frac{726}{125} c^{-m} = 0{\cdot}1277363$$

$$A_{11} = \frac{31}{625} + \frac{8359}{625} c^{-m} = 0{\cdot}0502072$$

$$A_{13} = 0{\cdot}0172805$$

$$A_{15} = 0{\cdot}0052779$$

$$A_{17} = 0{\cdot}0014467$$

$$A_{19} = 0{\cdot}0003593$$

$$A_{21} = 0{\cdot}0000815$$

$$A_{23} = 0{\cdot}0000170$$

$$A_{25} = 0{\cdot}0000036.$$

The horizontal refraction answers to $\cos \theta = 0$, $e = 1$; and the part of it depending on Q_0 is found by adding all the coefficients, viz.

$$\frac{a(1+a)}{\sqrt{5}\, i} \times 2{\cdot}8024736 = 2036''{\cdot}52.$$

If we take the integral between the limits $x = 0$, $x = \infty$, the result is not sensibly different, viz.

$$\alpha(1+\alpha)\int_0^\infty \frac{dx\,c^{-x}}{\sqrt{2ix}} = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} = 2036''{\cdot}52.$$

Investigation of $\lambda \times Q_1$.

For this purpose we must find the value of

$$\int_0^m \frac{2\,dx\,c^{-2x}}{\Delta} = \int_0^m \frac{2\,dx\,c^{-2x}}{\sqrt{(1-e^2)^2 + 4e^2\cdot\frac{x}{m}}} = \int_0^m \frac{2\,dx\,c^{-2x}}{\sqrt{(1-e^2)^2 + 4e^2\cdot\frac{2x}{2m}}};$$

this integral has therefore the same form as Q_0, the quantities $2x$ and $2m$ taking the place of x and m. Wherefore, if we assume

$$e \times \int \frac{2\,dx\,c^{-2x}}{\Delta} = a_1 e + a_3 e^3 + a_5 e^5 + \&c.,$$

the value of a_{2n+1} will be found merely by writing $2m$ for m in the expression of A_{2n+1}; but as $c^{-2m} = c^{-20}$ is extremely minute, the part multiplied by it may be neglected. Thus,

$$a_{2n+1} = 1 - n\cdot\frac{n+1}{2m} + n\cdot\frac{n-1}{2}\cdot\frac{n+1\,.\,n+2}{(2m)^2} - \&c.$$

The numerical coefficients are as follows:

$$\begin{aligned}
a_1 &= 1\\
a_3 &= 0{\cdot}9\\
a_5 &= 0{\cdot}73\\
a_7 &= 0{\cdot}535\\
a_9 &= 0{\cdot}3555\\
a_{11} &= 0{\cdot}21505\\
a_{13} &= 0{\cdot}118945\\
a_{15} &= 0{\cdot}0604215\\
a_{17} &= 0{\cdot}0283127\\
a_{19} &= 0{\cdot}0122898\\
a_{21} &= 0{\cdot}0049621\\
a_{23} &= 0{\cdot}0018695\\
a_{25} &= 0{\cdot}0006623.
\end{aligned}$$

These values being found, if we assume

$$\lambda \times Q_1 = B_3 e^3 + B_5 e^5 + B_7 e^7 + \&c.,$$

the term $\lambda \times c^{-m} \times e$, which is insensible, being omitted, we shall have

$$B_3 = \lambda(a_3 - A_3) = 0{\cdot}021866$$

$$B_5 = \lambda\ (a_5 - A_5) = 0{\cdot}045961$$
$$B_7 = \lambda\ (a_7 - A_7) = 0{\cdot}055760$$
$$B_9 = \lambda\ (a_9 - A_9) = 0{\cdot}049829$$
$$B_{11} = \lambda\ (a_{11} - A_{11}) = 0{\cdot}036064$$
$$B_{13} = \lambda\ (a_{13} - A_{13}) = 0{\cdot}022242$$
$$B_{15} = \lambda\ (a_{15} - A_{15}) = 0{\cdot}012064$$
$$B_{17} = \lambda\ (a_{17} - A_{17}) = 0{\cdot}005878$$
$$B_{19} = \lambda\ (a_{19} - A_{19}) = 0{\cdot}002610$$
$$B_{21} = \lambda\ (a_{21} - A_{21}) = 0{\cdot}001067$$
$$B_{23} = \lambda\ (a_{23} - A_{23}) = 0{\cdot}000405$$
$$B_{25} = \lambda\ (a_{25} - A_{25}) = 0{\cdot}000144.$$

By making $\cos\theta = 0$, $e = 1$, we shall have, for the approximate value of the part of the horizontal refraction depending on $\lambda\, Q_1$,

$$\frac{\alpha(1+\alpha)}{\sqrt{5i}} \times 0{\cdot}253891 = 184''{\cdot}50.$$

If the integrals be taken from $x = 0$ to $x = \infty$, the same quantity will be

$$\lambda \times \alpha(1+\alpha)\int \frac{dx\,(2c^{-2x} - c^{-x})}{\sqrt{2ix}} = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \cdot \lambda\,(\sqrt{2} - 1) = 184''{\cdot}56.$$

Between the two limits, the exact quantity obtained by integrating from $x = 0$ to $x = m = 10$, must lie; so that the error of the series is of no account.

It may be proper to make an observation here, which applies generally to the kind of integrals peculiar to this investigation. The first term of $\lambda\, Q_1$, viz. $B_1 e = \lambda\, c^{-m} \cdot e$, which is rejected, varies with the height of the atmosphere. If a small number be taken for m, that is, in low atmospheres, the refractions will vary with the height, and will not agree with the observed quantities; if a considerable number be taken, as eight or ten, or any greater number, that is, if the atmosphere extend forty or fifty miles or more above the earth's surface, the refractions will not be sensibly different from what they would be in an atmosphere of unlimited height. The invariability of the refractions concurs with other phenomena to prove that the air reaches an elevation of fifty miles, more or less.

Investigation of $f \times Q_2$.

We have

$$\frac{Q_2}{e} = \int_0^m \frac{dx}{\Delta}\left(4c^{-2x} - 3c^{-x} + xc^{-x}\right).$$

Now the following formula is easily proved by differentiating,

$$\int \frac{dx}{\Delta} x c^{-x} = \frac{1}{2}\int \frac{dx\,c^{-x}}{\Delta} - \frac{m}{4} \cdot \frac{(1-e^2)^2}{e^2} \cdot \int \frac{dx\,c^{-x}}{\Delta} + \frac{m}{4} \cdot \frac{1 - e^2 - c^{-x}\Delta}{e^2},$$

all the integrals vanishing when $x = 0$. By extending the integrals to $x = m = 10$, in which case $\Delta = 1 + e^2$, the result will be

$$\int_0^m \frac{dx}{\Delta} x c^{-x} = \frac{1}{2}\int_0^m \frac{dx c^{-x}}{\Delta} - \frac{5}{2}\cdot\frac{(1-e^2)^2}{e^2}\cdot\int_0^m \frac{dx c^{-x}}{\Delta} + \frac{5}{2}\cdot\frac{1-e^2+c^{-m}(1+e^2)}{e^2};$$

and, by substituting this value, we shall have

$$Q_2 = 2e\int_0^m \frac{2\,dx c^{-2x}}{\Delta} - \frac{5}{2}\int_0^m \frac{e\,dx c^{-x}}{\Delta} - \frac{5}{2}\cdot\frac{(1-e^2)^2}{e^2}\cdot\int_0^m \frac{e\,dx c^{-x}}{\Delta}$$
$$+ \frac{5}{2}\cdot\frac{1-c^{-m}}{e} - \frac{5}{2}(1+c^{-m})e.$$

The value of Q_2 will now be obtained in a series of the powers of e by putting for the integrals the equivalent series that have already been investigated. When this is done, the three first terms will be as follows:

$$\frac{5}{2}\left(1 - c^{-m} - A_1\right)\cdot\frac{1}{e}$$
$$+\left(2a_1 - \frac{5}{2}A_1 + 5A_1 - \frac{5}{2}A_3 - \frac{5}{2}(1+c^{-m})\right)\cdot e$$
$$+\left(2a_3 - \frac{5}{2}A_3 - \frac{5}{2}A_1 + 5A_3 - \frac{5}{2}A_5\right)\cdot e^3.$$

Upon substituting the exact values of A_1, A_3, &c., the first of these terms is zero: the other two are as follows:

$$-8c^{-m}\times e$$
$$+\frac{49}{5}c^{-m}\times e^3;$$

the amount of which is very small even at the horizon; and, when multiplied by $f = \frac{2}{9}$, it becomes insensible. These terms being neglected, we may assume

$$Q_2 = C_5 e^5 + C_7 e^7 + C_9 e^9 + \&c.;$$

and we shall find

$$C_5 = 2a_5 - \frac{5}{2}A_5 - \frac{5}{2}\Delta^2 A_3$$
$$C_7 = 2a_7 - \frac{5}{2}A_7 - \frac{5}{2}\Delta^2 A_5$$
$$\vdots$$
$$C_{2n+1} = 2a_{2n+1} - \frac{5}{2}A_{2n+1} - \frac{5}{2}\Delta^2 A_{2n-1}.$$

The numerical coefficients will now be obtained:

$$\Delta^2 A_1 = -\cdot 0802325$$
$$\Delta^2 A_3 = +\cdot 0403433 \ldots\ldots C_5 = \cdot 059337$$

$$\Delta^2 A_5 = \cdot0873930 \ldots\ldots C_7 = \cdot151186$$
$$\Delta^2 A_7 = \cdot0748672 \ldots\ldots C_9 = \cdot204491$$
$$\Delta^2 A_9 = \cdot0446024 \ldots\ldots C_{11} = \cdot193076$$
$$\Delta^2 A_{11} = \cdot0209241 \ldots\ldots C_{13} = \cdot142381$$
$$\Delta^2 A_{13} = \cdot0081714 \ldots\ldots C_{15} = \cdot087220$$
$$\Delta^2 A_{15} = \cdot0027438 \ldots\ldots C_{17} = \cdot046149$$
$$\Delta^2 A_{17} = \cdot0008096 \ldots\ldots C_{19} = \cdot021658$$
$$\Delta^2 A_{19} = \cdot0002133 \ldots\ldots C_{21} = \cdot009187$$
$$\Delta^2 A_{21} = \cdot0000511 \ldots\ldots C_{23} = \cdot003569$$
$$\Delta_2 A_{23} = \cdot0000105 \ldots\ldots C_{25} = \cdot001290.$$

As the value of f is not fixed with the same certainty as that of λ, the coefficients of Q_2 have not been multiplied by f: the intention of which is to make it more easy to determine a variation of the refraction, viz $\delta f \times Q_2$, answering to δf any variation of f that good observations may require.

The part of the horizontal refraction depending on Q_2 is

$$\frac{2}{9} \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times 0{\cdot}919534 = 148''{\cdot}51.$$

If we integrate the original expression of Q_2 from $x = 0$ to $x = \infty$, e being 1, we shall have

$$\frac{2}{9} \times \alpha(1+\alpha) \times \int \frac{dx(4c^{-2x} - 3c^{-x} + xc^{-x})}{\sqrt{2ix}} = \frac{2}{9} \cdot \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \cdot \left(2\sqrt{2} - \frac{5}{2}\right) = 148''{\cdot}63.$$

It thus appears that the error is less than $0''{\cdot}12$; for the exact integral from $x = 0$ to $x = m = 10$, is less than the second number, and greater than the first on account of the terms of the series left out.

The next point that should engage attention is to find the value of $f' \times Q_3$. In the present state of our knowledge of the phenomena of the atmosphere, it seems impossible to determine f' by experiments. The probability is, that it is much less than f or $\frac{2}{9}$; and as the integral Q_3 is inconsiderable except within a degree or two above the horizon, and even at such low altitudes is not great; it follows that the part of the refraction depending on $f' Q_3$ will only be sensible, if at all, when a star is distant 88° or more from the zenith. At present the probability is, that there is no other way of ascertaining the value of f' but by good observed refractions at great distances from the zenith; which observations are neither numerous nor easily collected. From the uncertainty of the term $f' \times Q_3$, it cannot be estimated in constructing a table of mean refractions, which must therefore be deduced entirely from the other three terms, as in the paper of 1823. In this manner has the table in this paper been computed, by means of the formulas now to be explained. But the term $f' Q_3$ will after-

wards be discussed, and its value investigated, in order that it may be taken into account, if this should be found necessary, in the progressive improvement of the theory.

When the term $f' Q_3$ is left out, the expression of the refraction will be

$$\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \cdot (Q_0 + \lambda Q_1 - f Q_2'):$$

and if the equivalent series be substituted for the first two terms, and the series for Q_2 be multiplied by $f = \frac{2}{9}$, the result will be

$$\begin{aligned}\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \Big\{ & e \\ & + 0{\cdot}821921 \,.\, e^3 \\ & + 0{\cdot}552697 \,.\, e^5 \\ & + 0{\cdot}302296 \,.\, e^7 \\ & + 0{\cdot}132123 \,.\, e^9 \\ & + 0{\cdot}043365 \,.\, e^{11} \\ & + 0{\cdot}007883 \,.\, e^{13} \\ & - 0{\cdot}002040 \,.\, e^{15} \\ & - 0{\cdot}002930 \,.\, e^{17} \\ & - 0{\cdot}001842 \,.\, e^{19} \\ & - 0{\cdot}000893 \,.\, e^{21} \\ & - 0{\cdot}000371 \,.\, e^{23} \\ & - 0{\cdot}000139 \,.\, e^{25}\end{aligned}$$

To bring this formula to a form more convenient for calculation, all the coefficients must be reduced to seconds. The negative terms are all very small, never amounting to so much as 6″, and of no account whatever, except the apparent altitude be equal to 2° or less; it will therefore be proper to separate these terms from the rest, representing their sum by the symbol V (θ). These things being attended to, we have, in the first place, this formula for computing e, viz.

$$\log . \tan\varphi = \log . \sec . \theta + 19{\cdot}2067840 - 20 : \; e = \tan\tfrac{\varphi}{2}.$$

Next, reducing the arcs to seconds,

$$\frac{\alpha(1+\alpha)}{\sqrt{5i}} = 726''{\cdot}687:$$

$$\begin{aligned}\delta\theta = \sin\theta \times \Big\{ & e \times 726''{\cdot}687, && \overset{\log.}{2{\cdot}8613472} \\ & + e^3 \times 597{\cdot}280, && 2{\cdot}7761772 \\ & + e^5 \times 401{\cdot}638, && 2{\cdot}6038343\end{aligned}$$

$$
\begin{aligned}
&+ e^7 \times 219{\cdot}674, && 2{\cdot}3417796\\
&+ e^9 \times \ 96{\cdot}012, && 1{\cdot}9823255\\
&+ e^{11} \times \ 31{\cdot}513, && 1{\cdot}4984866\\
&+ e^{13} \times \ \ 5{\cdot}728, && 0{\cdot}7580287\\
&- \mathrm{V}(\theta).
\end{aligned}
$$

$$
\mathrm{V}(\theta) = \sin\theta \times \left\{ \begin{array}{lll} e^{15} \times 1''{\cdot}483, & & \overset{\log.}{0{\cdot}1710}\\ + e^{17} \times 2{\cdot}129, & & 0{\cdot}3282\\ + e^{19} \times 1{\cdot}337, & & 0{\cdot}1266\\ + e^{21} \times 0{\cdot}649, & - & 1{\cdot}8122\\ + e^{23} \ \ 0{\cdot}270, & - & 1{\cdot}4307\\ + e^{25} \ \ 0{\cdot}102, & - & 1{\cdot}0072 \end{array} \right.
$$

When $\theta = 87°$, $\mathrm{V}(\theta)$ is zero; and if this function be computed for every succeeding half-degree, the quantity answering to any intermediate value of θ will be found by an easy interpolation. Such is the intention of the following Table; by the help of which any refraction from the zenith to the horizon may be computed by a series of the simplest form, and consisting of no more than seven terms.

θ.	$\mathrm{V}(3\theta)$
	$''$
$87\frac{1}{2}$	0·06
88	0·14
$88\frac{1}{2}$	0·38
89	0·86
$89\frac{1}{2}$	2·30
90	5·97

If $e = 1$, the result will be the horizontal refraction, viz.

$$2078''{\cdot}53 - 5''{\cdot}97 = 2072''{\cdot}56,$$

which is almost exactly the same with $2072''{\cdot}46$, the quantity before computed in § 10 by a very different method.

12. We next proceed to inquire into the influence which the term multiplied by f'', before omitted, may have on the refractions.

Investigation of the integral Q_3.

The expression of this integral is,

$$\mathrm{Q}_3 = \int_0^\infty \frac{e\,dx}{\Delta}\left(8\,c^{-2x} - 8\,c^{-x} + 7\,x\,c^{-x} - 2\,x^2\,c^{-x} + \frac{x^3}{6}\,c^{-x}\right),$$

which is a negative quantity, as appears from the valuation of it in § 9: it will therefore contribute to distinctness if its sign be changed, in which case it will be thus

written,

$$Q_3 = \int_0^m \frac{e\,dx}{\Delta}\left(-8c^{-2x} + 8c^{-x} - 7x\,c^{-x} + 2x^2 c^{-x} - \frac{x^3}{6}c^{-x}\right);$$

and the formula for the refractions will now be,

$$\delta\theta = \sin\theta \times \frac{a(1+a)}{\sqrt{5}i}\left(Q_0 + \lambda Q_1 - f Q_2 - f' Q_3\right).$$

Suppressing the tedious operations of reducing, we may put the integral Q_3, taken indefinitely, in the following form, which it is not difficult to verify by differentiating:

$$\epsilon = \frac{1-e^2}{e},$$

$$\begin{aligned} Q_3 = &-4\int\frac{e.2\,dx\,c^{-2x}}{\Delta} + \frac{91}{16}\int\frac{e\,dx\,c^{-x}}{\Delta} \\ &+\left(\frac{215}{16}\epsilon^2 + \frac{175}{16}\epsilon^4 + \frac{125}{48}\epsilon^6\right)\cdot\int\frac{e\,dx\,c^{-x}}{\Delta} \\ &+\frac{c^{-x}\Delta}{e}\left(\frac{185}{16} + \frac{125}{12}\epsilon^2 + \frac{125}{48}\epsilon^4\right) \\ &-\epsilon\left(\frac{185}{16} + \frac{125}{12}\epsilon^2 + \frac{125}{48}\epsilon^4\right) \\ &-\frac{c^{-x}\Delta}{e}\left(\frac{95}{24}x - \frac{5}{12}x^2 + \frac{25}{24}x\,.\,\epsilon^2\right). \end{aligned}$$

This being the indefinite integral, the value of Q_3 in the formula for the refractions will be obtained by putting $x = m = 10$; which gives

$$\frac{c^{-x}\Delta}{e} = \frac{1+e^2}{e}\cdot c^{-m};$$

and this value, as well as that of ϵ, being substituted, the quantity sought will be expressed as follows:

$$\begin{aligned} Q_3 = &-4\int\frac{e.2\,dx\,c^{-2x}}{\Delta} + \frac{91}{16}\int\frac{e\,dx\,c^{-x}}{\Delta} \\ &+\left\{\frac{215}{16}\left(\frac{1-e^2}{e}\right)^2 + \frac{175}{16}\left(\frac{1-e^2}{e}\right)^4 + \frac{125}{48}\left(\frac{1-e^2}{e}\right)^6\right\}\cdot\int\frac{e\,dx\,c^{-x}}{\Delta} \\ &+c^{-m}\left(\frac{125}{48}\frac{1}{e^3} - \frac{125}{16}\frac{1}{e^3} + \frac{905}{48}\cdot\frac{1}{e} + \frac{905}{48}e - \frac{125}{16}e^3 + \frac{127}{48}e^5.\right) \\ &-\frac{125}{48}\cdot\frac{1}{e^3} + \frac{125}{48}\frac{1}{e^3} - \frac{305}{48}\frac{1}{e} + \frac{305}{48}e - \frac{125}{48}e^3 + \frac{125}{48}e^5. \end{aligned}$$

The series equivalent to the integrals must now be substituted, in order to express the quantity sought in terms containing the powers of e.

In the first place we have these three terms, each of which is zero when the exact values of A_1, A_3, &c. are substituted, viz.

$$\frac{125}{48}(A_1 - 1 + c^{-m})\cdot\frac{1}{e}$$

$$+\left\{\frac{175}{16}A_1+\frac{125}{48}(A_3-6A_1)+\frac{125}{48}-\frac{125}{16}c^{-m}\right\}\cdot\frac{1}{e^3}$$

$$+\left\{\frac{215}{16}A_1+\frac{175}{16}(A_3-4A_1)+\frac{125}{48}(A_5-6A_3+15A_1)-\frac{305}{48}+\frac{905}{48}c^{-m}\right\}\times\frac{1}{e^5}$$

The next three terms are as follows:

$$\left\{-4a_1+\frac{91}{16}A_1+\frac{215}{16}(A_3-2A_1)+\frac{175}{16}(A_5-4A_3+6A_1)\right.$$
$$\left.+\frac{125}{48}(A_7-6A_5+15A_3-20A_1)+\frac{305}{48}+\frac{905}{48}c^{-m}\right\}\cdot e$$

$$+\left\{-4a_3+\frac{91}{16}A_3+\frac{215}{16}(A_5-2A_3+A_1)\right.$$
$$+\frac{175}{16}(A_7-4A_5+6A_3-4A_1)$$
$$+\frac{125}{48}(A_9-6A_7+15A_5-20A_3+15A_1)$$
$$\left.-\frac{125}{48}-\frac{125}{16}c^{-m}\right\}\cdot e^3$$

$$+\left\{-4a_5+\frac{91}{16}A_5+\frac{215}{16}(A_7-2A_5+A_3)\right.$$
$$+\frac{175}{16}(A_9-4A_7+6A_5-4A_3+A_1)$$
$$+\frac{125}{48}(A_{11}-6A_9+15A_7-20A_5+15A_3-6A_1)$$
$$\left.+\frac{125}{48}+\frac{125}{48}c^{-m}\right\}\cdot e^5.$$

On substituting the exact values of A_1, A_3, &c., these three terms will come out as follows:

$$+\frac{158}{3}c^{-m}\cdot e, \qquad \text{or } +\cdot00239\cdot e$$

$$-\frac{348}{5}c^{-m}\cdot e^3, \qquad \text{or } -\cdot00316\cdot e^3$$

$$+\frac{8891}{75}c^{-m}\cdot e^5, \qquad \text{or } +\cdot00538\cdot e^5.$$

These three terms are the part of the refraction that depends on the height of the atmosphere: at the horizon, or when $e=1$, their amount is greatest and equal to

$$f'\times\frac{\alpha(1+\alpha)}{\sqrt{5i}}\times\cdot00461=f'\times726''\cdot7\times\cdot00461=f'\times3''\cdot3,$$

which, on account of the smallness of f', will be a minute fraction of a second.

Rejecting the six foregoing terms, we may assume

$$Q_3=H_7e^7+H_9e^9+H_{11}e^{11}+\&c.:$$

and, having computed the differences in the following table,

	Δ^3	Δ^4	Δ^5
A_1	.....	.....	+·0278859
A_3	.....	−·0595755	−·0175110
A_5	+·0873930	−·0177390	−·0199864
A_7	+·0748672	+·0065865	−·0079396
A_9	+·0446024	+·0109256	−·0002312
A_{11}	+·0209241	+·0073251	+·0016762
A_{13}	+·0081714	+·0034934	+·0012515
A_{15}	+·0027438	+·0013379	+·0005925
A_{17}	+·0008096	+·0004339	+·0001891
A_{19}	+·0002133	+·0001224	.....
A_{21}	+·0000509	.....	.....

we shall have

$$H_7 = -4a_7 + \frac{91}{16}A_7 + \frac{215}{16}\Delta^2 A_5 + \frac{175}{16}\Delta^4 A_3 + \frac{125}{48}\Delta^6 A_1 = \cdot 04861$$

$$H_9 = -4a_9 + \frac{91}{16}A_9 + \frac{215}{16}\Delta^2 A_7 + \frac{175}{16}\Delta^4 A_5 + \frac{125}{48}\Delta^6 A_3 = \cdot 07091$$

$$H_{11} = -4a_{11} + \frac{91}{16}A_{11} + \frac{215}{16}\Delta^2 A_9 + \frac{175}{16}\Delta^4 A_7 + \frac{125}{48}\Delta^6 A_5 = \cdot 04469$$

$$H_{13} = -4a_{13} + \frac{91}{16}A_{13} + \frac{215}{16}\Delta^2 A_{11} + \frac{175}{16}\Delta^4 A_9 + \frac{125}{48}\Delta^6 A_7 = \cdot 00249$$

$$H_{15} = -4a_{15} + \frac{91}{16}A_{15} + \frac{215}{16}\Delta^2 A_{13} + \frac{175}{16}\Delta^4 A_{11} + \frac{125}{48}\Delta^6 A_9 = -\cdot 02230$$

$$H_{17} = -4a_{17} + \frac{91}{16}A_{17} + \frac{215}{16}\Delta^2 A_{15} + \frac{175}{16}\Delta^4 A_{13} + \frac{125}{48}\Delta^6 A_{11} = -\cdot 02558$$

$$H_{19} = -4a_{19} + \frac{91}{16}A_{19} + \frac{215}{16}\Delta^2 A_{17} + \frac{175}{16}\Delta^4 A_{15} + \frac{125}{48}\Delta^6 A_{13} = -\cdot 01835$$

$$H_{21} = -4a_{21} + \frac{91}{16}A_{21} + \frac{215}{16}\Delta^2 A_{19} + \frac{175}{16}\Delta^4 A_{17} + \frac{125}{48}\Delta^6 A_{15} = -\cdot 01023$$

$$H_{23} = -4a_{23} + \frac{91}{16}A_{23} + \frac{215}{16}\Delta^2 A_{21} + \frac{175}{16}\Delta^4 A_{19} + \frac{125}{48}\Delta^6 A_{17} = -\cdot 00487.$$

The coefficients of the assumed series being found, and being expressed in seconds of a degree, the part of the refractions depending on Q_3 will be as follows:

$$f' \times \sin\theta \times \frac{a(1+a)}{\sqrt{5i}} \times Q_3 = f' \times \sin\theta \times \left\{ \begin{array}{lll} & & \text{log.} \\ & e^7 \times 35''\cdot 324, & 1\cdot 54807 \\ + & e^9 \times 51\cdot 529, & 1.71205 \\ + & e^{11} \times 32\cdot 476, & 1\cdot 51156 \\ + & e^{13} \times \ 1\cdot 809, & 0\cdot 25755 \\ - & e^{15} \times 16\cdot 205, & 1\cdot 20965 \\ - & e^{17} \times 18\cdot 588, & 1\cdot 26925 \end{array} \right.$$

$$\left.\begin{array}{lll} -e^{19} \times 13''\cdot334, & \log. & 1\cdot12498 \\ -e^{21} \times 7\cdot427, & & 0\cdot87080 \\ -e^{23} \times 3\cdot480, & & 0\cdot54158 \end{array}\right\}$$

The amount of this expression at the horizon, or when $e = 1$, is $f' \times 62''\cdot1$, almost the same with $f' \times 62''\cdot4$, which, as is shown in § 9, is the limit of the integral when it is extended from $x = 0$ to $x = \infty$. It is thus proved that the error of the series is of no account. This part of the refraction cannot be computed because f' is unknown. But although the precise value of f' is uncertain, it is probably very considerably less than f, or $\frac{2}{9}$; so that the effect on the refraction cannot exceed a few seconds even at the horizon. We shall be better able to form a just notion with respect to this point, when the Theoretical Table in this paper is compared with observations.

13. It remains to investigate the corrections that must be made in the practical application for the deviations indicated by the meteorological instruments from the mean constants used in constructing the table.

For this purpose we have

$$\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times S,$$

$$S = Q_0 + \lambda Q_1 - f Q_2,$$

$$\frac{\sqrt{5i}}{\cos\theta} = \frac{e}{1-e^2},$$

$$\lambda = \frac{\alpha}{i}.$$

The quantities e and λ depend only upon α and i: α varies both with the barometer and thermometer, and i, with the thermometer only: the quantity f does not seem liable to change in our climate. Admitting that the prefix d refers only to variations of the barometer and thermometer, we shall have

$$\delta\theta + d.\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \left\{ \left(1 + \frac{d\alpha}{\alpha} - \frac{1}{2}\cdot\frac{di}{i}\right).S + \frac{de}{e}\cdot\frac{dS}{de}e + \frac{d\lambda}{\lambda}\cdot\lambda Q_1 \right\}.$$

Now

$$\frac{de}{e} = \frac{1}{2}\cdot\frac{di}{i}\cdot\frac{1-e^2}{1+e^2},$$

$$\frac{d\lambda}{\lambda} = \frac{d\alpha}{\alpha} - \frac{di}{i}:$$

wherefore

$$\delta\theta + d.\delta\theta = \delta\theta\left(1 + \frac{d\alpha}{\alpha}\right)$$
$$- \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5}i} \cdot \frac{di}{i}\left(\frac{S}{2} - \frac{1}{2} \cdot \frac{1-e^2}{1+e^2} \cdot \frac{dS}{de}\, e\right)$$
$$+ \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5}i} \cdot \left(\frac{d\alpha}{\alpha} - \frac{di}{i}\right) . \lambda Q_1.$$

If p denote the observed height of the barometer, reduced to the fixed temperature of 50° of FAHR.; and τ the temperature of the air on the same scale; then, $\beta = \frac{1}{480}$,

$$1 + \frac{d\alpha}{\alpha} = \frac{1}{1+\beta(\tau-50)} \cdot \frac{p}{30},$$
$$\frac{d\alpha}{\alpha} = -\frac{\tau-50}{480} - \frac{30-p}{30},$$
$$\frac{di}{i} = +\frac{\tau-50}{480},$$
$$\frac{d\alpha}{\alpha} - \frac{di}{i} = -2 \times \frac{\tau-50}{480} - \frac{30-p}{30}.$$

These values being found, if we put

$$T = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5}i} \times \frac{1}{480} \times \left(\frac{(1+e^2)S - (1-e^2)\frac{dS}{de}\, e}{2(1+e^2)} + 2\lambda Q_1\right),$$
$$b = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5}i} \times \frac{2\lambda Q_1}{30};$$

the expression of the mean refraction with its correction will be as follows,

$$\delta\theta + d.\delta\theta = \frac{\delta\theta}{1-\beta(\tau-50)} \cdot \frac{p}{30} - T.(\tau-50) - b(30-p).$$

The first term of this expression is the mean refraction corrected in the manner usually practised by Astronomers. If we assume that the temperature of the mercury in the barometer is the same with that of the air, this term will be equal to

$$\frac{1}{1+\beta(\tau-50)} \cdot \frac{1}{1+\frac{\tau-50}{10000}} \cdot \frac{p}{30} = \frac{1}{1+c(\tau-50)} \cdot \frac{p}{30},$$

$$c = \cdot 002183,$$

the new factor being added to compensate the expansion of the mercury. Two subsidiary tables are given for computing this part: Table II. contains the logarithms of $\frac{1}{1+c(\tau-50)}$ for 30° on either side of the mean temperature 50°, negative indices being avoided by substituting the arithmetical complements; and Table III. contains the logarithms, or the arithmetical complements, for all values of p from 31 to 28.

The coefficients, T and b, of the other two terms vary with the distance from the

zenith; and they can be computed in no other way than by reducing them to series of the powers of e. By substituting for λQ_1, the equivalent series already known, we immediately obtain

$$b = \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5}\,i} \cdot \frac{1}{30} \cdot \left\{ B_3 e_3 + B_5 e^5 + B_7 e^7 + \&c. \right\}.$$

Further, by expanding S and its differential, the expression of T will take this form,

$$T = \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5}\,i} \cdot \frac{1}{480} \cdot \left\{ G_3 e^3 + G_7 e^7 + G_9 e^9 + \&c. \right\};$$

and we shall have

$G_3 = A_1 - A_3 + 2B_3 = 0{\cdot}2436$

$G_5 = -A_1 + 3A_3 - 2A_5 + 2B_5 = 0{\cdot}4523$

$G_7 = A_1 - 3A_3 + 5A_5 - 3A_7 + 2B_7 = 0{\cdot}4705$

$G_9 = -A_1 + 3A_3 - 5A_5 + 7A_7 - 4A_9 + 2B_9 = 0{\cdot}3502$

$G_{11} = A_1 - 3A_3 + 5A_5 - 7A_7 + 9A_9 - 5A_{11} + 2B_{11} = 0{\cdot}2092$

$G_{13} = -A_1 + 3A_3 - 5A_5 + 7A_7 - 9A_9 + 11A_{11} - 6A_{13} + 2B_{13} = 0{\cdot}1050.$

The series for T and b being now known, the coefficients of the terms must next be expressed in seconds of a degree; which being done, the following final results will be obtained.

$T = \sin\theta \times \{$		log.	$b = \sin\theta \times \{$		log.
e^3	$''0{\cdot}369,$	$-1{\cdot}5668,$	e^3	$''0{\cdot}530,$	$-1{\cdot}7240$
$+ e^5$	$0{\cdot}685,$	$-1{\cdot}8356$	$+ e^5$	$1{\cdot}113,$	$0{\cdot}0465$
$+ e^7$	$0{\cdot}712,$	$-1{\cdot}8526$	$+ e^7$	$1{\cdot}350,$	$0{\cdot}1306$
$+ e^9$	$0{\cdot}530,$	$-1{\cdot}7263$	$+ e^9$	$1{\cdot}207,$	$0{\cdot}0817$
$+ e^{11}$	$0{\cdot}317,$	$-1{\cdot}5006$	$+ e^{11}$	$0{\cdot}873,$	$1{\cdot}9412$
$+ e^{13}$	$0{\cdot}159,$	$-1{\cdot}2013$	$+ e^{13}$	$0{\cdot}539,$	$-1{\cdot}7313$

The values of T and b are added in separate columns of the annexed table for altitudes less than 10°: for greater altitudes they are omitted as of no account. The application for finding the corrected refraction from the formula

$$\delta\theta + d.\delta\theta = \frac{\delta\theta}{1 + c(\tau - 50)} \cdot \frac{p}{30} - T(\tau - 50) - b(30 - p),$$

will best be explained by the examples afterwards given.

14. The Theoretical Table of refractions which has been computed by the foregoing formulas, and which is deduced solely from the phenomena of the atmosphere without arbitrary assumptions, is next to be compared with the tables most esteemed by astronomers. Two tables more eminently deserve this character; namely, BESSEL's table with its supplement in the *Tabulæ Regiomontanæ*, which may be considered as the result of observations, and as being nearly exact to 88° or 88°½ from the zenith; and

the table published annually in the *Connaissance des Temps*. As all the tables are supposed to contain the same series of refractions, the numbers corresponding to the same altitude should have constantly the same proportion: so that taking the number α which answers to the zenith-distance θ in Bessel's table, the logarithm of the refraction at the same zenith distance in the New Table should be equal to

$$\log \alpha + \log \tan \theta + \cdot 00507,$$

the number ·00507 being the difference of the logarithms of the refractions at the altitude of 45° in the two tables: but, in the supplemental table, which contains the logarithms of the refractions, it is sufficient to add ·00507 to obtain the logarithms in the New Table. With regard to the refractions in the *Conn. des Temps*, it is more convenient to use the Table in the *Tables Astronomiques* published by the French Board of Longitude: for the logarithms in this table with the addition of ·0011, should agree respectively with the logarithms of the New Table. According to these directions the following comparative view has been drawn up.

Zenith dist.	Refractions.		
	New Table.	Tab. Reg.	Conn. des Temps.
°			
10	10″·30	10″·30	10″·30
20	21·26	21·26	21·26
30	33·72	33·72	33·72
40	48·99	48·99	48·99
45	58·36	58·36	58·36
50	69·52	69·52	69·52
55	83·25	83·24	83·25
60	100·85	100·85	100·86
65	124·65	124·62	124·65
70	159·16	159·11	159·22
75	214·70	214·58	214·83
80	320·19	319·88	320·63
81	353·79	353·38	354·33
82	394·68	394·20	395·37
83	445·42	444·86	445·87
84	509·86	509·23	511·22
85	593·96	593·38	595·80
85½	646·21	647·10	648·34
86	707·43	707·15	710·07
86½	779·92	777·36	783·07
87	866·76	864·59	870·37
87½	971·93	972·21	975·89
88	1101·35	1101·40	1105·1
88½	1262·6	1265·5	1265·0
89	1466·8	1481·8	1464·9
89½	1729·5	1764·9	1716·4

From this view it appears that the three Tables agree within less than 1″ as far as 80° from the zenith; the New Table is in accordance with Bessel's, with slight discrepancies, to 88° or 88½° from the zenith; from 80° to 88° of zenith distance the numbers in the French Table exceed those in Bessel's, the excess being 2″ at 84°, and 4″ at 88°. But when the distance from the zenith is greater than 80°, the accuracy of

the French Table is questionable, both on account of the hypothetical law of the densities, and because the quantity assumed for the horizontal refraction is uncertain.

A few examples are subjoined, as well for explaining the use of the New Table as for affording some indications of its accuracy at low altitudes. The two first instances are taken from the *Tables Astronomiques*, and are likewise published yearly in the *Conn. des Temps*.

Example 1.

$\theta = 86° 14' 42''$
Therm. 8°·75 cent. = 47°·75 F.
Barom. 0^m·741 = 29·17 in.

86° 10′	2·86345
4 42″	664
	2·87009
Therm.	·00214
Barom.	9·98781
Log $\delta\theta$	2·86004
$\delta\theta$	724·5
$-\cdot25 \times -2\frac{1}{4}$	$+\cdot5$
$-\cdot4 \times \cdot8$	$-\cdot3$
Corrected refraction	12′ 4″·7
Observed refraction	12 4 ·2

Example 2.

$\theta = 86° 15' 20''$
Therm. 8$\frac{1}{2}$° cent. = 46°·9 F.
Barom. 0^m·766 = 30·16 in.

86° 10′	2·86345
5 20″	753
	2·87098
Therm.	·00276
Barom.	·00232
Log $\delta\theta$	2·87606
$\delta\theta$	751·7
$-\cdot25 \times -3\cdot1$	$+\cdot8$
$-\cdot4 \times -\cdot16$	$+\cdot6$
Corrected refraction	12′ 33″·1
Observed refraction	12 32 ·5

Example 3.

Mean of 42 sub-polar observations of α Lyræ by Dr. Brinkley.
Irish Transactions, 1815.
$\theta = 87° 42' 10''$
Therm. 35°
Barom. 29·5

87° 40′	3·00522
2 10″	·392
	3·00914
Therm.	·01444
Barom.	9·99270
Log $\delta\theta$	3·01628
$\delta\theta$	1038″·2
$-\cdot6 \times -15$	$+9\cdot0$
$-1\cdot13 \times \frac{1}{2}$	$-0\cdot6$
Corrected refraction	17′ 26″·6
Observed refraction	17 26 ·5

Example 4.

Mean of 10 observations of Capella, from a memoir of M. Plana.
Acad. de Turin, tom. 32.
$\theta = 88° 24' 9''·7$
Therm. 47°·75
Barom. 29·75

88° 20′	3·08087
4 9″·4	847
	3·08934
Therm.	·00214
Barom.	9·99607
Log $\delta\theta$	3·08755
$\delta\theta$	1223″·3
$-\cdot95 \times -2\frac{1}{4}$	$+2\cdot1$
$-1\cdot6 \times \cdot27$	$-0\cdot4$
Corrected refraction	20′ 25″
Observed refraction	20 24 ·3

We may now inquire how far the refractions are likely to be affected by the term which it was found necessary to leave out, because the present state of our know-

ledge of the phenomena of the atmosphere made it impossible to determine the coefficient f' by which it is multiplied. For this purpose the term alluded to, viz.

$$\sin\theta \times f' \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times Q_3,$$

which may be shortly denoted by $f' \times \chi(\theta)$, has been computed by means of the equivalent series, for every half degree between 85° and 88°, the results being as follows:

θ	$f' \times \chi(\theta)$
85°	$f' \times$ 1″·5
85½	$f' \times$ 2·0
86	$f' \times$ 3·3
86½	$f' \times$ 4·9
87	$f' \times$ 7·4
87½	$f' \times$ 11·2
88	$f' \times$ 17·0

From this view it appears that f', although considerably less than f or $\frac{2}{9}$ may still have some influence on the refractions at very low altitudes. The mean refraction in Bessel's Table, and in the New Table, can hardly be supposed to differ 2″ from the true quantity, which would limit f' to be less than $\frac{1}{10}$. It is a matter of some importance to obtain a near value of f': and it is probable that this can be accomplished in no other way but by searching out such values of f and f' as will best represent many good observed refractions at altitudes less than 5°. If such values were found, our knowledge of the decrease of heat in ascending in the atmosphere would be improved, and the measurement of heights by the barometer would be made more perfect.

April 25th, 1838.

Table I.

Mean Refractions for the Temperature 50° Fahrenheit, and the barometric Pressure 30 inches.

Zenith dist.	ϑ A.	Log ϑ A.	Diff.	T.	C.	Zenith dist.	ϑ A.	Log ϑ A.	Diff.	T.	C.
°	′ ″					°	′ ″				
1	0 1·02	0·0085	3012			53	1 17·38	1·88863	1577		
2	2·04	0·3097	1763			54	20·24	1·90440	1596		
3	3·06	0·4860	1252			55	23·25	1·92036	1617		
4	4·08	0·6112	974			56	26·41	1·93653	1638		
5	5·11	0·7086	796			57	29·73	1·95291	1664		
6	6·14	0·7882	675			58	33·23	1·96955	1691		
7	7·17	0·8557	587			59	36·93	1·98646	1722		
8	8·21	0·9144	519			60	40·85	2·00368	1756		
9	9·25	0·9663	466			61	45·01	2·02124	1794		
10	10·30	1·0129	424			62	49·44	2·03918	1836		
11	11·35	1·0553	388			63	54·17	2·05754	1881		
12	12·42	1·0941	359			64	59·23	2·07635	1932		
13	13·49	1·1300	334			65	2 4·65	2·09567	1988		
14	14·57	1·1634	313			66	10·48	2·11555	2048		
15	15·65	1·1947	294			67	16·78	2·13603	2116		
16	16·75	1·2241	278			68	23·61	2·15719	2191		
17	17·86	1·2519	265			69	31·04	2·17910	2275		
18	18·98	1·2784	252			70 00	39·16	2·20186	388		
19	20·11	1·3036	241			10	40·59	2·20573	390		
20	21·26	1·3277	230			20	42·04	2·20963	393		
21	22·42	1·3507	222			30	43·52	2·21356	396		
22	23·60	1·3729	215			40	45·02	2·21752	398		
23	24·80	1·3944	207			50	46·53	2·22150	402		
24	26·01	1·4151	201			71 00	48·08	2·22552	404		
25	27·24	1·4352	195			10	49·65	2·22956	407		
26	28·49	1·4547	189			20	51·25	2·23363	410		
27	29·75	1·4736	185			30	52·87	2·23773	413		
28	31·05	1·4921	181			40	54·53	2·24186	417		
29	32·38	1·5102	177			50	56·21	2·24603	419		
30	33·72	1·5279	173			72 00	57·92	2·25022	423		
31	35·09	1·5452	170			10	59·66	2·25445	425		
32	36·49	1·5622	168			20	3 1·43	2·25870	429		
33	37·93	1·5790	164			30	3·23	2·26299	433		
34	39·39	1·5954	162			40	5·06	2·26732	436		
35	40·89	1·6115	160			50	6·93	2·27168	440		
36	42·42	1·6276	159			73 00	8·83	2·27608	443		
37	44·00	1·6435	156			10	10·77	2·28051	447		
38	45·61	1·6591	155			20	12·74	2·28498	450		
39	47·27	1·6746	155			30	14·75	2·28948	454		
40	48·99	1·6901	154			40	16·80	2·29402	458		
41	50·75	1·7055	152			50	18·88	2·29860	462		
42	52·57	1·7207	151			74 00	21·01	2·30322	467		
43	54·43	1·7358	152			10	23·18	2·30789	470		
44	56·35	1·7510	151			20	25·39	2·31259	475		
45	58·36	1·76611	1512			30	27·66	2·31734	479		
46	1 0·43	1·78123	1514			40	29·95	2·32213	483		
47	2·57	1·79637	1518			50	32·30	2·32696	488		
48	4·80	1·81155	1523			75 00	34·70	2·33184	493		
49	7·11	1·82678	1530			10	37·16	2·33677	497		
50	9·52	1·84208	1539			20	39·65	2·34174	502		
51	12·02	1·85747	1551			30	42·21	2·34676	507		
52	14·64	1·87298	1565			40	44·82	2·35183	512		
53	17·38	1·88863				50	47·48	2·35695			

Table I. (Continued.)

Zenith dist.	$\Delta\delta$.	Log. $\Delta\delta$.	Diff.	T.	C.	Zenith dist.	$\Delta\delta$.	Log $\Delta\delta$.	Diff.	T.	C.
75° 50	3′ 47″·48	2·35695				83° 00	7′ 25″·42	2·64877			
			517						833		
76 00	50·21	2·36212	523			10	35·09	2·65810	948		
10	53·00	2·36735	528			20	45·14	2·66758	970		
20	55·85	2·37263	533			30	55·64	2·67728	985		
30	58·76	2·37796	538			40	8 6·55	2·68713	1005		
40	4 1·74	2·38334	545			50	17·95	2·69718	1028		
50	4·79	2·38879	551			84 00	29·86	2·70746	1047	·10	·16
77 00	7·91	2·39430	557			10	42·31	2·71793	1069		
10	11·11	2·39987	563			20	55·33	2·72862	1092		
20	14·39	2·40550	569			30	9 8·96	2·73954	1116		
30	17·74	2·41119	576			40	23·25	2·75070	1140		
40	21·19	2·41695	583			50	38·23	2·76210	1166		
50	24·72	2·42278	589			85 00	53·96	2·77376	1194	·15	·24
78 00	28·33	2·42867	596			10	10 10·52	2·78570	1219		
10	32·04	2·43463	603			20	27·90	2·79789	1248		
20	35·84	2·44066	611			30	46·21	2·81037	1281		
30	39·75	2·44677	618			40	11 5·55	2·82318	1312		
40	43·76	2·44295	626			50	25·90	2·83626	1342		
50	47·88	2·45921	635			86 00	47·43	2·84968	1377	·24	·39
79 00	52·12	2·46556	642			10	12 10·21	2·86345	1412		
10	56·47	2·47198	650			20	34·34	2·87757	1448		
20	5 0·94	2·47848	659			30	59·92	2·89205	1488	·31	·51
30	5·54	2·48507	669			40	13 27·11	2·90693	1627		
40	10·28	2·49176	677			50	55·99	2·92220	1570		
50	15·16	2·49853	688	·03	·04	87 00	14 26·76	2·93790	1612	·39	·67
80 00	20·19	2·50541	696			10	59·54	2·95402	1658	·43	·75
10	25·36	2·51237	707			20	15 34·55	2·97060	1704	·47	·83
20	30·70	2·51944	716			30	16 11·93	2·98764	1758	·52	·91
30	36·20	2·52660	727			40	52·10	3·00522	1808	·58	1·01
40	41·88	2·53387	738			50	17 35·12	3·02330	1862	·63	1·13
50	47·74	2·54125	749	·04	·05	88 00	18 21·35	3·04192	1918	·69	1·24
81 00	53·79	2·54874	761			10	19 11·07	3·06110	1977	·78	1·41
10	6 0·04	2·55635	772			20	20 4·68	3·08087	2040	·87	1·58
20	6·50	2·56407	785			30	21 2·60	3·10127	2102	·96	1·75
30	13·18	2·57192	797			40	22 5·22	3·12229	2169	1·07	2·00
40	20·09	2·57989	811			50	23 13·11	3·14398	2239	1·19	2·24
50	27·26	2·58800	824	·05	·08	89 00	24 26·8	3·16637	2316	1·32	2·48
82 00	34·68	2·59624	838			10	25 46·8	3·18943	2388	1·52	2·91
10	42·37	2·60462	851			20	27 14·2	3·21331	2461	1·72	3·34
20	50·33	2·61313	866			30	28 49·5	3·23792	2529	1·92	3·77
30	58·59	2·62179	883			40	30 33·2	3·26321	2573	2·20	4·34
40	7 7·19	2·63062	899			50	32 15·1	3·28894		2·48	5·00
50	16·13	2·63961	916	·07	·11	90 00	34 32				
83 00	25·42	2·64877									

Table II.

Thermometer.					
	Log.	Diff.		Log.	Diff.
50°	0·00000		50°	0·00000	
49	0·00094		51	9·99906	
48	0·00190		52	9·99811	
47	0·00285		53	9·99717	
46	0·00380		54	9·99623	
45	0·00476	96	55	9·99529	
44	0·00572		56	9·99434	94
43	0·00668		57	9·99341	
42	0·00764		58	9·99248	
41	0·00861		59	9·99154	
40	0·00957		60	9·99061	
39	0·01053		61	9·98969	
38	0·01151	98	62	9·98875	
37	0·01248		63	9·98783	92
36	0·01346		64	9·98690	
35	0·01444		65	9·98598	
34	0·01541		66	9·98506	
33	0·01640		67	9·98414	
32	0·01738		68	9·98323	
31	0·01837		69	9·98231	
30	0·01935		70	9·98140	
29	0·02033		71	9·98049	
28	0·02133	100	72	9·97958	
27	0·02232		73	9·97867	
26	0·02331		74	9·97777	90
25	0·02432		75	9·97686	
24	0·02531		76	9·97596	
23	0·02630		77	9·97506	
22	0·02730		78	9·97416	
21	0·02832	102	79	9·97326	
20	0·02933		80	9·97237	

Table III.

Barometer.		
	Log.	Diff.
In.		
31	0·01424	
30·9	0·01248	
8	0·01143	
7	0·01002	142
6	0·00860	
5	0·00718	
4	0·00575	
3	0·00432	144
2	0·00289	
1	0·00145	
30·0	0·00000	
29·9	9·99855	
8	9·99709	
7	9·99563	
6	9·99417	146
5	9·99270	
4	9·99123	
3	9·98975	148
2	9·98826	
1	9·98677	
29·0	9·98528	150
28·9	9·98378	
8	9·98227	
7	9·98076	
6	9·97924	
5	9·97772	152
4	9·97620	
3	9·97466	
2	9·97313	
1	9·97158	
28·0	9·97004	154

X. *Researches on the Tides.—Ninth Series. On the Determination of the Laws of the Tides from short Series of Observations.* By the Rev. W. Whewell, *M.A., F.R.S., Fellow of Trinity College, Cambridge.*

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1. The discussion of tide observations, for the purpose of obtaining from them the laws of the phenomena, has hitherto been usually conducted upon the supposition that a series of several years in succession was requisite, in order that the accidental irregularities might disappear in the *means* of the observations, and the effects of the lunar inequalities thus come clearly into view. But in the present memoir I shall endeavour to show with what degree of exactness the laws of the phenomena, and the effects of the lunar inequalities, may be deduced from shorter series of observations; for example, from series of one year.

2. I conceive that such an investigation will be of value in several ways. If the principal elements of tide tables for each particular place can be obtained with moderate accuracy from short series of observations properly discussed, the formation of such independent tide tables for different places and times will become far less laborious than it has hitherto been deemed, and may be expected to be far more commonly practised. This will be a great advantage, not only because the tide tables will thus become better, but also because several important questions may thus be settled; for instance, whether, and how far, the laws of tide phenomena change from place to place, and from time to time. These questions we are as yet unable to answer with confidence or with accuracy, although they affect the very foundations of all tide theory, as well as the permanent value of tide tables.

3. But there is another consideration which makes it desirable to compare the results of short series with those of long ones. Without such a comparison we cannot appreciate the *practical* accuracy of our tide tables. If, for instance, the mean of nineteen years of tide observations gave a very exact rule for the effect of lunar parallax, while each single year deviated widely from this rule, it would be clear that the individual observations must be commonly affected by casual irregularities considerably greater than the parallax correction; and therefore the practical accuracy of the tables would be very little improved by introducing into them the parallax correction. I hope to establish, on the contrary, in the following pages, that the general law, and the approximate amount, of the parallax correction, may be traced in the observations of a single year; and thus, that the tables are rendered practically

as well as theoretically better, by such a correction being employed. The same is true, though in a less marked manner, of the correction for lunar declination.

4. It is hardly necessary to remark, as an additional recommendation of the mode of discussing tide observations which this memoir contains, that it leads us, quite as well as any other method, to the results of longer series. When we have obtained the table or the curve of the parallax correction for several single years in succession, the more accurate table or curve which the whole group of years would give is the mean of the single instances. We thus obtain all the accuracy which a longer series of years can supply, while we have, at the same time, the advantage of seeing how much each year contributes to this accuracy.

6. In obtaining the laws of tide phenomena from the observations, different modes of procedure may be adopted, and it still remains to be decided which of these modes is the best. For instance, instead of referring each tide to the moon's transit immediately preceding, we may refer it to the transit one day, or a day and a half, or two days anterior; and it is important to determine which of these modes of reference represents most closely the laws of the phenomena. It is also desirable to ascertain what is the nature and amount of the variations, which these alterations of the epoch of the lunitidal interval introduce into the correction tables for declination and parallax. To settle this point has been one object of the present Researches; and for that purpose I have calculated the correction tables for Bristol, referring the tide to the lunar transit one day, one day and a half, and two days anterior to the transit immediately preceding the tide. Of the results of these different hypotheses I shall hereafter speak.

7. I will add that the present memoir, by clearing up such points as I have noticed, appears to be suited to wind up the series of general researches respecting the tides, which I have, during some years, laid before the Royal Society from time to time. For if methods of discussing tide observations at any place be given, so complete, that the general laws of the corrections may be easily obtained from the observations of a single year, and good tables from a few years, there remains nothing to be done except to apply these methods to good observations, and thus to construct and improve our tide tables for all the most important places; an employment which can easily be carried on by those persons who have performed the calculations on which the present and preceding memoirs are founded. And thus the study of the tides might be pursued, and, to do the subject justice, ought to be pursued, in the same manner as the study of the other provinces of astronomy: that is, constant and careful observations should be made of the phenomena; and, as fast as they are made, should be reduced and discussed at the public expense; so as to test the accuracy of the tables already obtained, and to supply the means of making them still more accurate. In this manner also, any new corrections, and any changes in the elements of the old corrections, would be brought into view as soon as there was evidence of

their existence. Till the subject of the tides is thus treated in a manner worthy of its scientific importance, and of the promise which it now holds forth, it must be considered as offering a blot in that system of the national cultivation of astronomy, of which our public observatories are, in other departments of science, such effective and magnificent examples.

8. The reference of the phenomena of the tides to mechanical principles by rigorous reasoning has not yet been executed; and we can see enough of the difficulties of the subject to believe that it will probably be some time before this problem can be fully solved. In the meanwhile, we have an intermediate type of comparison for the facts, in the equilibrium theory of Bernoulli: for that theory, by modifying the epochs and other elements, may be made to represent in an approximate manner the laws of the phenomena. Nor does it appear to be too much to expect, that it may hereafter be rigorously shown from mechanical principles, that the form of an irregular moving fluid mass, constantly dragged along by certain forces, shall at every instant resemble the form of equilibrium which the forces would produce at some anterior epoch, the anterior epoch being somewhat different for the different features of the fluid form. If such a hydrodynamical proposition could be established, almost all the facts hitherto discovered respecting the tides would be fully explained.

9. The materials of the following researches are the tide observations made at Plymouth in the years 1834, 1835, 1836, and 1837, under the direction of the Dock Master and Assistant Dock Master; and the observations made at Bristol in 1834, 1835, 1836, 1837, by direction of the Dock Committee.

The Plymouth observations were discussed by Mr. Dessiou and Mr. D. Ross of the Hydrographer's Office, whose valuable services were placed at my disposal by the Hydrographer, Captain Beaufort. The Bristol observations were discussed by Mr. Bunt, whom I was enabled to engage in this employment by means of grants voted for that purpose at the Meetings of the British Association for the Advancement of Science which took place in 1836 and 1837.

10. The methods of discussing the observations which were employed were somewhat different for the two places. As that practised at my suggestion by Mr. Bunt for the Bristol tides appears to be the better of the two, I will subjoin his description of the process.

I may previously observe that in all cases we employ the method already described in previous papers: namely, upon a series of parallel ordinates* corresponding to the times of moon's transit, we lay down the successive tides, that is, the heights or the lunitidal intervals, as the one or the other are the subject of examination. This curve is more or less irregular, but for most places the leading feature is the zigzag form which arises from the diurnal inequality†. A curve is drawn by the eye so as

* For the purpose of all these operations it is most convenient to have paper ruled into small squares of inches and twelfths of inches.

† Researches, Seventh Series, Philosophical Transactions, 1837.

to cut off this inequality, leaving equal differences above and below. We may then proceed as follows to find the other inequalities.

"Having laid down the observed intervals and heights, referring both to the *apparent* time of the moon's transit, and drawn through each series of points the dotted line which cuts off the diurnal inequality only, but retains every other, I trace off on a piece of transparent paper, having an axis drawn on it extending from $0^h\ 0^m$ to $12^h\ 0^m$ transit, the successive portions of the dotted line of observation just mentioned, which are included between those hours during the first three months of the year; fitting the tracing paper in its place every time by means of the two extreme points of the axis. I thus obtain six irregular curves, the mean of which is found by drawing across them, at equal distances, twenty-four vertical lines, and finding by my scale, a point in each which is the exact mean of the six intersections. In this manner I get four mean curves on separate pieces of paper, which by repeated combinations are reduced into one, being the mean semimenstrual curve for the year.

"The next step is to reduce this curve to a mean parallax (57″·2) at each hour of transit. For this purpose an arrangement must be made, showing the mean parallax for that year at each of the twelve hours, which will be found to vary from about 56″·9 to 57″·5. Thus the mean parallax for 1836, at the several hours of transit, is

Hour of Transit...	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
Parallax.....	57″·5	57″·4	57″·3	57″·1	57″·0	56″·9	56″·9	57″·0	57″·1	57″·2	57″·4	57″·5
Mean of all 57″·2.												

"The parallax table of the preceding year, if already discussed, will be sufficiently near for making the requisite small alteration of the curve to the mean parallax: otherwise an approximate parallax table for the current year must be first made. I then calculate the mean declination, which varies, not only as the parallax, slightly from hour to hour, but also considerably from year to year. The hourly differences (being only about half a degree from the mean) I have disregarded, and I prefer marking on each annual curve the mean declination of that year, to any attempt to reduce the different annual curves to one common declination. Having thus very carefully obtained the mean semimenstrual curve, I cut it out nicely on a piece of thick drawing-paper, and laying the intersections of the vertical hour lines of 0^h and 12^h with the axis, on the corresponding point, on my sheets, I pencil in the mean curves, both of time and of height, and then ink them. The residue, or space between the mean curve and that of observation is next transferred to a straight line below.

"In examining this residue, the first step is to lay on an *approximate* line of parallax. This I take from the parallax correction-curves of the preceding year, if it has been discussed. If not, I make an arrangement for every hour of transit, and for 54′, 55′, 56′, and 59′, 60′, 61′ of parallax (omitting 57′ and 58′), of all the vertical distances, at the successive hours of transit, of the curve from the straight line, adding

to each a constant, to avoid negative quantities. I thus obtain twenty-four parcels, the means of which give an approximate correction for $55' \ldots 60'$ (or rather $55' \ldots 59\frac{1}{4}'$) at every hour of transit. From this a first line of parallax is laid down, preparatory to the obtaining of the declination corrections.

"This is done by collecting the measured distances from the parallax line to the curve, into parcels of 0° decl. to 13° decl., and 21° decl. to 28° decl. for every hour of transit, omitting those of 14° to 20° decl., and taking the axis, with the mean declination of the year, as a better representative of the mean declinations. The means of these twenty-four parcels give me a declination correction very near the truth. From this I lay down, on the parallax line, the effect of declination, and thus get an approximate curve of declination combined with parallax. Both this and the former curve of parallax are drawn in with pencil only, being merely used as approximations, whence *corrections* of the first parallax and declination *corrections* are obtained. This is done by an arrangement (as before) of the spaces still remaining between the pencil curve of declination combined with parallax, for hours of transit, and $54' \ldots 56'$, $59' \ldots 61'$ parallax, which gives a small additional correction of the parallax corrections first obtained. From this corrected parallax correction I draw in, with ink, the true parallax line, making the requisite alteration in the line of declination combined with parallax, which is then also inked in. Finally, a *correction* of the declination *corrections* being made from this latter line, is considered as giving the true effect of the declinations.

"A further improvement has been introduced into the discussions of 1836, which ought not to be neglected. In former arrangements, e. g. of the twelve parcels of observations with the parallaxes $54'$, $55'$, $56'$, the mean of the *measured distances only* was taken, the mean of the *corresponding parallaxes* having been all assumed $= 55'$, whereas they differ sometimes half a minute. I have now taken, in all cases, both the mean of the observed quantities and the mean of the parallaxes, or declinations corresponding to that particular parcel; these are afterwards reduced to a common value.

"The above description applies equally to the two processes, for time and for height, which are conducted both in the same manner."

11. By the methods thus described, we obtain curves which represent the semimenstrual inequality and the corrections for lunar parallax and declination, both of the times and of the heights, for any assumed parallaxes and declinations both below and above the mean. By operating in this manner upon the observations of each year separately, we obtain curves for each year; and by combining these for a sufficient number of years, we obtain a mean result, which will possess greater accuracy. From these curves, and especially from the mean of them, we can construct tables of the corrections, which may be used in calculating predictions of the tides, such as are commonly called Tide Tables.

12. But though this is undoubtedly one main use of a discussion of tide observations,

our labours were conducted, as I have already stated, with a peculiar view to several questions respecting the connection of tide phenomena at different times and places; and I will now state the answers to these questions, which our results supply.

I. *To which transit of the moon ought we to refer the tide?*

It has been well shown by Mr. Lubbock that the agreement between the empirical laws of the tides and the equilibrium theory is much improved, by referring the tides to a transit anterior to their occurrence by two or three days. He denotes the successive transits of the moon (at intervals of about twelve hours) by the letters A, B, C, D, E, F; F being the transit which immediately precedes (by about two hours) the high water at London: and he finds that the laws of the tides at London and Liverpool agree best with theory when they are referred to the transit B. The tide which reaches London at two hours after the transit F, was at Plymouth about six hours after the transit E; and as from transit B to transit E is about thirty-eight hours, if we refer the Plymouth tides to transit B, we take a transit about forty-four hours before the tide. Transits A and C are about fifty-six and thirty-two hours anterior to the tide. Nearly the same may be said of Bristol, for the tide there is nearly contemporaneous with that at Plymouth. I have, therefore, referred the Bristol tides of 1834 and 1835 to each of the three transits A, B, C, and have compared the results.

The general result is, that the transit B gives the best tables. This is shown both by the unaccounted-for residue of the observed quantities, which is smallest when transit B is used; and by the form of the curves for the parallax and declination corrections, which is most regular for this transit.

We may therefore, it would seem, assume, at least for our own coasts, that the tides are to be referred to a transit of the moon, which takes place a day and a half before the tide reaches the coasts of Europe.

We may, however, observe that we do not in this way obtain an exact agreement of observation and theory, even with regard to the semimenstrual inequality. It has appeared from Mr. Lubbock's researches respecting the Liverpool tides*, that while the transit A gives a very exact agreement of the theoretical and observed times, we must take a still earlier transit if we would obtain this agreement with respect to the heights. Nor does that selection of a transit which best represents the semimenstrual inequality, bring about an agreement with theory in the parallax and declination corrections, as we shall see. We must allow, therefore, that though there appear to be, in the actual laws of the tides, inequalities *corresponding* to all these which arise from the supposition of the equilibrium-tide of an anterior epoch transmitted along the ocean to our shores, we cannot so assume the epochs to produce all the inequalities at once. The epoch is of one value for the times, of another for the heights; different again for the parallax correction, and again different for the effect of declination.

* Philosophical Transactions, 1836, Part II.

II. *How does a change of the epoch affect the semimenstrual inequalities?*

The moon's transit takes place every half-day about twenty-four minutes later by solar time. The semimenstrual inequality, both of interval and of height, is referred to the solar time of the moon's transit. The height is the same whether it be referred to the transit E immediately preceding, or to the transit B, which is a day and a half sooner. Also if the moon moved uniformly in her orbit, the *inequality* of the interval of the tide and the transit would be the same, whether the tide were referred to the transit E, or to any anterior one, as B; for the interval is increased by a constant quantity (twelve hours twenty-four minutes) for every transit that we go back. But though the inequality of the interval for any given tide would be the same, it would not occupy the same place in the table or curve, since it would be referred to a different hour of transit. For example, if the mean interval of transit and tide at Bristol, referred to transit E, be 7^h, and if when the moon's transit is 2^h, the tide be at 8^h, the interval is 6^h, and therefore in this case the inequality is *minus* one hour. If now we refer this tide to the transit B, a day and a half sooner, the interval of the transit and tide will be $6^h + 36^h + 72^m$, and the mean interval will be $7^h + 36^h + 72^m$; and therefore, as before, the inequality is *minus* one hour. But the moon's transit E taking place at 2^h, solar time, the transits D, C, B will take place at $1^h\ 36^m$, $1^h\ 12^m$, $0^h\ 48^m$ respectively, and therefore the inequality of minus one hour, which was referred to the transit happening at 2^h when transit E was used, is referred to the transit at $0^h\ 48^m$, when we employ the anterior epoch B.

We thus see, that by referring to an anterior epoch, the whole semimenstrual inequality is moved backwards through twenty-four minutes of lunar transit, for every step of one transit backwards. This is the mean result; supposing the moon's motion to be uniform, and neglecting all other inequalities; on which suppositions nothing would be gained or lost in accuracy by the change of epoch. In the actual case we shall find this mean result modified by the influence of the other inequalities, which make one transit a better epoch than another.

The transfer of the semimenstrual inequality curve which we are thus led to expect on changing the epoch, shows itself in the results of the Bristol observations. For example, the time of transit for which the semimenstrual inequality of the interval is 0, being taken for the three transits A, B, C, we find

	For A.	For B.	For C.
In 1834,	$6^h\ 4^m$;	$6^h\ 28^m$;	$6^h\ 52^m$.
In 1835,	$5^h\ 58^m$;	$6^h\ 22^m$;	$7^h\ 4^m$.

Again, for the semimenstrual inequality of heights we find that the minimum height corresponds to the following hours of moon's transit:

	For A.	For B.	For C.
In 1834,	$5^h\ 30^m$;	$5^h\ 54^m$;	$6^h\ 24^m$.
In 1835,	$5^h\ 29^m$;	$5^h\ 46^m$;	$6^h\ 18^m$.

The maximum height corresponds to the following hours of moon's transit:

	For A.	For B.	For C.
In 1834,	$11^h\ 27^m$;	$11^h\ 57^m$;	$12^h\ 28^m$.
In 1835,	$11^h\ 25^m$;	$11^h\ 47^m$;	$12^h\ 7^m$.

We may observe, that if we could find an epoch which should make the results exactly correspond with the theory, the semimenstrual inequality of the time would be 0^h for the time of transit 6^h; and the height would have its maximum and minimum values for the times of transit 6^h and 12^h. From the above results it follows, by an easy calculation, that the epoch which makes the inequality in time agree with the theory is about the transit A; and the epoch which makes the inequality of heights agree with the theory is about four hours after the transit B. We have therefore here still clearer evidence, of that which the discussion of the Liverpool tides by Mr. Lubbock sufficiently showed, that there is no one anterior epoch which will reduce the observed tides to an agreement with the equilibrium theory.

III. *How does a change of the epoch affect the (lunar) parallax correction of the time?*

The parallax corrections of the times, calculated with different epochs, have a general agreement with each other, and with the theory. Taking the correction for parallax 60′ as our example, it has a maximum and a minimum which correspond to transits differing only about three hours from each other; so that the inequality increases from its minimum to its maximum through about nine hours of transit, and then diminishes more rapidly from the maximum to the minimum through three hours. In this all the empirical curves and the theoretical curve agree. But in general the empirical curves differ in two respects from the theoretical one; first, according to theory, the transit which gives the maximum and that which gives the minimum are equally distant from 6^h; one being as much before as the other after that period; whereas in the observation, this mean point corresponds to $6\frac{1}{2}^h$ or 7^h transit; and secondly, the empirical correction contains, besides the variable part, a constant part which depends on the parallax, but does not vary with the hour of transit. This I had already noticed in the second series of these Researches*.

It follows from what is there stated, that while theory gives the formula†

$$(\mathrm{P} - p)\ (b \sin 2\phi),$$

observation gives such a formula as

$$(\mathrm{P} - p)\ (a + b \sin 2\ (\phi - \beta)).$$

But the quantity a as well as β, changes with the epoch, as Mr. Lubbock has justly observed; and the question occurs, whether we can, by a proper assumption of the epoch, cause the quantity a to disappear, and thus render the curve which represents the observed law symmetrical with regard to the axis, as the theoretical curve is.

* Philosophical Transactions, 1834, p. 40.

† Here P is the parallax, p the mean parallax, ϕ the hour of moon's transit, β a time depending on the epoch.

This question our discussion of the Bristol tides enables us to answer. For if we take the parallax correction for 60′, for the year 1834, and for the transit A, we find that it has a maximum 16^m at $5\frac{1}{4}^h$ transit; a minimum $= 1^m$ at $7\frac{3}{4}^h$ transit. Hence it may be represented by $8\frac{1}{3}^m - 7\frac{1}{3}^m \cdot s, 2\varphi - 13^h$, where s, 2φ represents a function of 2φ, which differs from sin 2φ in having its maximum and minimum at a smaller distance than six hours ($2\frac{1}{2}$ hours in this case), the distance depending on some other quantity*; but which, like sin 2φ, is 0 when φ is 0, + when φ is less than 6^h, − when φ is greater than 6^h; and has for its maximum and minimum values + 1 and − 1, at equal distances from the value $2\varphi = 0$.

In the same manner we may reduce to formulæ of the same kind the parallax corrections for the other transits. And putting them together we have the following results.

		m	m	h
Year 1834.	Transit A.	$8\frac{1}{3}$ −	$7\frac{1}{3} \cdot s, 2\varphi$ −	13
	Transit B.	$10\frac{3}{4}$ −	$12\frac{1}{4} \cdot s, 2\varphi$ −	14
	Transit C.	$3\frac{1}{3}$ −	$7\frac{1}{3} \cdot s, 2\varphi$ −	14
Year 1835.	Transit A.	$9\frac{1}{2}$ −	$7\frac{1}{3} \cdot s, 2\varphi$ −	$13\frac{1}{3}$
	Transit B.	$8\frac{1}{3}$ −	$10\frac{1}{3} \cdot s, 2\varphi$ −	14
	Transit C.	$2\frac{1}{2}$ −	$9\frac{1}{2} \cdot s, 2\varphi$ −	14

These results show, that by taking a later transit the quantity a is diminished, although irregularly; and therefore the epoch which would reduce the correction to the symmetry of theory is later than C; but we cannot pretend to say with precision how much, without further calculation. We may observe that the circumstance of the coefficients of the variable part being largest for the transit B, appears to indicate that the discussion relative to that transit is the best-conditioned for bringing into view the parallax inequality.

The results of the Bristol observations for 1836 and 1837, discussed for transit B, give, for the parallax correction of the times,

$$\text{for 1836, } 6^m + 9^m \cdot s, 2\varphi - 14^h;$$
$$\text{for 1837, } 11^m + 12^m \cdot s, 2\varphi - 13^h.$$

The Plymouth observations for 1834, 1835, 1836, 1837, referred to the transit B, give a parallax correction, which follows nearly the same laws as those just stated for Bristol. It has a maximum of 22^m at 5^h, and a minimum of -3^m at 9^h. Hence its formula is (for 61′ parallax)

$$9\tfrac{1}{2}^m - 12\tfrac{1}{2}^m \cdot s, 2\varphi - 14^h,$$

which agrees very nearly with the Bristol formula for transit B.

IV. *How does a change of the epoch affect the* (*lunar*) *declination correction of the times?*

* The expression $\frac{\sin 2(\varphi - \beta)}{1 + c \cos 2(\varphi - \beta)}$ is an instance of such an expression, the maximum and minimum being equally distant from the value $\varphi = \beta$, and each of these distances depending upon the value of c.

Observations similar to those which have been made concerning the correction of the times for parallax, may be repeated with respect to the declination. The theoretical correction is of the form*

$$(\sin^2 \delta - \sin^2 \Delta)\, d \,.\, s,\, 2\varphi;$$

but observation gives

$$(\sin^2 \delta - \sin^2 \Delta)\, (c + d \,.\, s,\, 2\varphi - 2\beta).$$

And the quantity c varies with a change of the epoch.

The following are the results of the Bristol observations.

For the year 1834, transit A, we find for the declination 7°, a maximum 7^m at $4\frac{1}{4}^h$; a minimum -7^m at $7\frac{3}{4}^h$. We find also for the declination 24°, a minimum -3^m at 5^h; a maximum 9^m at $7\frac{1}{4}^h$. Hence the corrections for declination of this series may be expressed by these formulæ:

$$\text{for } 7°,\ 0^m - 7^m \,.\, s,\, 2\varphi - 12^h;\ \text{for } 24°,\ 3^m + 6^m \,.\, s,\, 2\varphi - 12\tfrac{1}{4}^h.$$

Collecting in the same manner the corrections for the other series, we have

		Decl. 7°.	Decl. 24°.
		m m h	m m h
Year 1834.	Transit A,	$0 - 7 \,.\, s,\, 2\varphi - 12$,	$3 + 6 \,.\, s,\, 2\varphi - 12\frac{1}{4}$
	B,	$-\frac{1}{2} - 5\frac{1}{2} \,.\, s,\, 2\varphi - 12\frac{1}{2}$,	$\frac{1}{2} + 7\frac{1}{2} \,.\, s,\, 2\varphi - 13$
	C,	$5 - 5 \,.\, s,\, 2\varphi - 14$,	$-2\frac{3}{4} + 5\frac{1}{4} \,.\, s,\, 2\varphi - 13\frac{3}{4}$
Year 1835.	Transit A,	$1\frac{1}{4} - 8\frac{1}{4} \,.\, s,\, 2\varphi - 12$,	$2\frac{1}{4} + 5\frac{3}{4} \,.\, s,\, 2\varphi - 12$
	B,	$1\frac{1}{2} - 7\frac{1}{2} \,.\, s,\, 2\varphi - 13\frac{1}{4}$,	$-1\frac{1}{2} + 7\frac{1}{2} \,.\, s,\, 2\varphi - 13$
	C,	$7 - 11 \,.\, s,\, 2\varphi - 14$,	$-1 + 7 \,.\, s,\, 2\varphi - 14$

We here see that the transits A and B make c very small, so that when they are used, the declination correction curve approaches very near to the symmetry of theory. Also these epochs make the curve cut the axis very nearly at 6^h, as the theory gives.

The observations at Bristol for 1836 and 1837 give for the declination corrections, transit B being used,

	Decl. 7°.	Decl. 24°.
	m m h	m m h
1836,	$1\frac{1}{2} - 7\frac{1}{2} \,.\, s,\, 2\varphi - 13\frac{3}{4}$;	$\frac{1}{2} + 5\frac{1}{2} \,.\, s,\, 2\varphi - 12$;
1837,	$3\frac{3}{4} - 7\frac{3}{4} \,.\, s,\, 2\varphi - 12\frac{3}{4}$;	$-1\frac{3}{4} + 7\frac{1}{2} \,.\, s,\, 2\varphi - 12$.

The Plymouth observations for 1834, 1835, 1836, 1837, referred to transit B, give in like manner for the low declination 6°, a curve which has a maximum 10^m at 5^h, and a minimum -3^m at $8\frac{1}{2}^h$. Hence its formula would be

$$3\tfrac{1}{2}^m - 6\tfrac{1}{2}^m \,.\, s,\, 2\varphi - 13\tfrac{1}{2}^h.$$

The correction for 24° has a minimum of -9^m at 4^h, a maximum of 4^m at 8^h. Hence its formula is

$$-2\tfrac{1}{2}^m + 6\tfrac{1}{2}^m \,.\, s,\, 2\varphi - 12^h.$$

V. *How does a change of the epoch affect the parallax correction of heights?*

* Here δ is the declination, and Δ the mean declination.

The parallax correction of heights, according to theory, is of the form

$$(P - p)\,(a + b\,.\,s,\, 2\varphi).$$

This agrees with the Bristol observations, which give

$$(P - p)\,(a + b\,.\,s,\, 2\varphi - 2\beta);$$

for the amount of this correction for a given parallax (as 60′, for example,) is nearly constant for all hours of transit, having, however, a maximum about $6\frac{1}{2}^h$ and a minimum about $8\frac{1}{2}^h$. Hence β is about $7\frac{1}{2}^h$ in this case; the ratio of b to a, which is that of the solar to the lunar tide in theory, appears to be smaller in fact. Indeed for transit A the quantity b vanishes or becomes very small, and the parallax correction is nearly constant for all hours. This appears to show that the transit B is under better conditions than A for obtaining this correction.

The Plymouth parallax correction of heights follows nearly the same law. It has a maximum about $5\frac{1}{2}^h$ and a minimum about $8\frac{1}{2}^h$.

VI. *How does a change of the epoch affect the declination correction of the heights?*

The declination correction of heights as collected from the Bristol observations is somewhat irregular, the curve for the high and the low declinations being in some cases different in form. It does not appear that the change of epoch very much modifies these forms, at least in any steady manner. For all the epochs (transit A, B, and C) the correction for low declinations (as 7°) has a maximum about $6\frac{1}{2}^h$, which diminishes both ways, so as to make a protuberance in the curve from about 4^h to 8^h, the rest of the correction being nearly constant. For the high declinations (as 24°) the curve, in the best discussions (those of 1836 and 1837), appears to have nearly the same form as for the low declinations.

The Plymouth observations of this correction offer a minimum about 4^h, and a maximum about 7^h.

VII. *Does the parallax correction of height vary as the parallax?*

In the course of the discussions of the Plymouth tides, the parallax correction of height was taken for each of the twelve hours of transit, and laid down by means of ordinates corresponding to the minutes of transit from 54′ to 61′. The result was, for each year separately (1834–7), very nearly the same straight line, inclined to the axis, and cutting the axis about 57′, which is the mean value of the parallax. The mean of all these years does not differ sensibly from a straight line for each hour; thus showing that the parallax correction of the height does, in reality, vary in proportion to the parallax itself, as according to the theory it ought to do.

We may remark, however, as appears from the same discussion, that from 5^h to 9^h transit the highest parallaxes are wanting; and that for these hours the mean parallax is smaller than 57′, and the correction line cuts the axis before 57′, whereas for the other hours the point of intersection is after 57′. This result arises in a great measure from the moon's variation, which, increasing her distance from the earth at

quadrature, diminishes the parallaxes in that part of her orbit, and makes their greatest values less than they are in other situations.

The Bristol observations give the same result, which was established by our examination in a manner somewhat different. In 1836 the sum of the parallax corrections of height for all the twelve hours, from the parallax 60′ to the mean 57′·2, was 315 parts; from 57′·2 to 55′ it was 246 parts; the simple ratio of the parallaxes would give 315 to 247·5.

VIII. *Does the parallax correction of time vary as the parallax?*

The observations just referred to enable us to answer this question in the affirmative. In 1836 the corrections of time from 60′ to 57′·2 were 125, and from 57′·2 to 55′ they were 96 parts; the simple ratio of the parallaxes gives 125 to 98·2.

IX. *Does the declination correction of heights vary as the square of the declination?*

According to the theory, the correction for declination, both in the time and the height, will vary as the square of the declination, or as the square of its sine nearly, the correction being supposed to be applied to the declination 0°. And it was shown in the examination of the London tides*, that this is the law which prevails with tolerable regularity in the mean of nineteen years' observations, both for the times and heights. We may perhaps consider this rule therefore as established by much better evidence than two or three years' observations can supply. Still it is not unimportant to examine how far this rule manifests itself in the result of short series of observations. The Plymouth tides were examined with reference to this point. The diurnal, semimenstrual, and parallax corrections being determined and subtracted, the residues of the heights were for each hour expressed by ordinates corresponding to the degrees of declination (taken in groups 0°..3°, 3°..9°, 9°..15°, 15°..21°, 21°..28°). It appeared that in each year (from 1834 to 1837) the curve thus obtained was nearly a straight line, and it was still more nearly so in the mean of the years. This result appeared to show that the declination correction of heights is proportional to the declination itself, and not to its square. But the Bristol observations give a different result. Thus in 1835 the correction in height (for all the twelve hours together) from decl. 7°·3 to mean decl. 18°·6 was 192 parts; and from 18°·6 to 24°·4 it was 173 parts. This proportion of 192 to 173 is very nearly agreeable to the proportion of the squares of the declinations; indeed it is a still higher ratio; for the proportion of the squares gives 192 to 164, while the simple ratio of the declination would give 192 to 98. In 1836 the correction in height from decl. 8° to the mean 19°·7 was 227 parts; and from 19°·7 to 25° it was 138. The law of the squares would require the proportion 227 to 166, the law of the simple ratio, 227 to 103. In this case the result is nearly intermediate between the square and the simple ratio, but the mean of the two years gives the law of the square of the declination very nearly. And this result, agreeing with the London mean of nineteen years, may be considered as sufficiently established;

* Researches, Second Series, Philosophical Transactions, 1834, pp. 24. 32.

the occasional deviations in short series arising from the irregularity of the phenomena, and from the imperfection of the other corrections.

X. *Does the declination correction of times vary as the square of the declination?*

The Bristol observations give

1835. Total time correction from decl. 7°·3 to 18°·6; 81 parts
from — 18°·6 to 24°·4; 75 —

The ratio of the squares is 81 to 69; the simple ratio is 81 to 41.

1836. Total time correction from decl. 8° to 19°·7; 76 parts
from — 19°·7 to 25; 46 —

The ratio of the squares is 76 to 56; the simple ratio is 76 to 34. In both these cases the actual result is in a ratio considerably higher than the simple ratio; and the mean approaches near to the ratio of the square.

The Plymouth tides of 1835, 1836, 1837, were discussed in a different manner, the declination corrections for each hour being kept separate. The effects due to declination being thrown into curves according to the degrees of declination, the form of these curves from 2^h to 6^h transit appeared to show very clearly, that at those hours the correction is as the square of the declination; while for the other hours of transit the correction is small and irregular. It has already been shown that the declination correction of times is of the form $(\sin^2\delta - \sin^2\Delta)\ (c + d.s, 2\varphi - 2\beta)$, and the form of the irregularity appears to indicate that β is smaller for the high than for the low declinations. It may perhaps be possible hereafter to put this correction in a form which shall escape this irregularity.

XI. *Can the laws of the corrections be deduced from a single year?*

Whether the observations of a single year, or of a few years, suffice for determining the tide corrections, is a question which the discussions just described enable us to answer. For if the curves representing the laws of these corrections be regular for each year and similar in successive years, we cannot doubt that the law is given with some approximation by each, although the mean may be still more accurate. On examining our curves with this view we find that most of the corrections are given with tolerable precision, even by a single year's observations. The semimenstrual inequality may be determined from that period, and gives a very regular curve if the observations are good. It may be still improved by reducing each hour to the parallax and declination which is the mean of the whole, as I shall further explain. The parallax correction from a single year is less regular, as might be expected, the observations for each hour of transit being further distributed to each minute of parallax, so that each resulting number is the mean of a few observations only. Still the general form of the parallax correction curve for one year exhibits the features which belong to the mean; namely, a maximum and minimum about 4^h and 8^h both for time and height. And when the process is conducted with care, and the curves which represent the corrections adjusted nearer and nearer to the observations, in

the manner which the discussion itself suggests, the results may be rendered far more exact than would have been conceived possible. This was done by Mr. Bunt in the discussions of the Bristol tides for 1836 and 1837, and the result is that the form of the parallax correction deduced from the observations above, and from those below the mean, is almost identical. Without using any arbitrary improvement of the curves, they are absolutely symmetrical above and below the axis. The same is the case with the curves which express the corrections for high and for low declinations. The above methods applied to a single year appear to give as good forms of the correction curves as were at first obtained from the whole mass of nineteen years' observations. It is clearly shown by our researches, that a series of five years will give, with great regularity, the laws and amounts of all the corrections, especially if we attend to what we now know of their general form, and of the cycles of longer period by which they are affected.

XII. *Are there any regular differences between the corrections of successive years?*

The mean parallax of one year is very nearly the same as the mean parallax of another year*. But the case is different with the declination. The inclination of the moon's orbit continues nearly the same during one year, but varies from year to year in consequence of the revolution of the nodes. When the ascending node of the moon's orbit is in the first point of Aries, the inclination of the orbit to the equator is the sum of its inclination to the ecliptic, and of the obliquity of the ecliptic. When the descending node is at the first point of Aries, the inclination to the equator is the difference of the other two. In this manner the inclination of the moon's orbit to the equator may vary from about $18\frac{1}{2}°$, which value it had in 1829 and 1830, to $28\frac{1}{2}°$, which is its value in the present year 1838. Consequently the mean declination of the moon for different years will be different, and the semimenstrual curve obtained by taking the mean of the year will correspond to different declinations in different years. The mean declination for 1834, obtained by taking the correction to be proportional to the square, is, for 1834, 17°·2; for 1835, it is 18°·6; for 1836 it is 19°·7; for 1837 it is 20°·2. Hence on this account alone the semimenstrual curve of 1837 and 1834 would differ. For instance, for 8^h transit, the difference of effect of declination 17°·2 and 20°·2 is 6^m or 8^m; and hence the semimenstrual curves will deviate from each other by such a quantity. Accordingly this feature appears in the results of the Bristol observations; and we have in this fact a very remarkable evidence that the effects of declination are discoverable in the tides of each year.

The Bristol observations for 1834, 1835, 1836, and 1837 being reduced, according to this view, to a common parallax and declination, give the following results, from which the agreement of different years may be judged of.

* The mean parallax of the different hours of transit is not the same: the greatest parallax occurring near the syzygies, in consequence of the moon's variation, as I have already observed. Hence the semimenstrual curve obtained by taking the mean of each hour of transit, requires a correction for parallax to reduce it to the general mean.

Hour of Transit.....	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
Times. Semimenstrual Inequality. Mean Parallax 57′·2. Mean Declination 19°.												
Interval.	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
1834. (Mean Interval 43ʰ 56ᵐ)	44 3	43 47	43 30	43 15	43 8	43 21	43 57	44 34	44 44	44 42	44 34	44 19
1835. (Mean Interval 43ʰ 36ᵐ)	44 8	43 48	43 29	43 13	43 7	43 21	44 2	44 34	44 44	44 42	44 33	44 21
1836. (Mean Interval 43ʰ 56ᵐ)	44 4	43 45	43 28	43 15	43 6	43 19	44 4	44 36	44 47	44 44	44 35	44 22
1837. (Mean Interval 43ʰ 56ᵐ)	44 7	43 47	43 29	43 13	43 5	43 20	44 2	44 35	44 47	44 45	44 37	44 24
Mean of 1834–7	44 5·5	43 46·7	43 29	43 14	43 6·5	43 20·2	44 1·2	44 34·7	44 45·5	44 43·2	44 34·7	44 21·5
Greatest Difference	2·5	1·7	1·0	1·0	1·5	1·2	4·2	1·3	1·5	1·8	2·3	2·5
Heights. Semimenstrual Inequality. Mean Parallax 57′·2. Mean Declination 19°.												
	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
1834.	31·6	31·0	29·3	26·9	24·3	23·1	22·3	25·0	27·6	29·6	31·0	31·7
1835.	31·7	30·7	28·8	26·5	24·0	22·2	22·8	25·3	27·8	29·7	31·1	31·7
1836.	31·5	30·7	29·1	26·8	24·1	22·0	22·5	25·0	27·6	29·6	31·0	31·6
1837.	31·7	30·7	29·0	26·8	24·2	22·0	22·3	24·8	27·4	29·8	31·2	31·8
Mean of 1834–7	31·6	30·8	29·0	26·8	24·1	22·1	22·4	25·0	27·6	29·7	31·1	31·7
Greatest Difference	0·14	0·25	0·22	0·21	0·14	0·09	0·32	0·03	0·20	0·11	0·10	0·13

It appears from these Tables that the greatest difference of the semimenstrual inequality of time in any one year from the mean of the four years is $4\frac{1}{2}$ minutes; and the greatest difference of height in the same manner is about 4 inches. It is to be recollected that this is the error in a mean tide of 33 feet.

XIII. *Do the corrections at different places agree in laws and amount?*

The amount of agreement in this respect will be seen by the following statement.

The semimenstrual inequality in time, although according to theory it is determined by the proportion of the force of the moon to that of the sun, is by the observations different at different places. The total amount of this inequality, that is, the difference of the greatest and least lunitidal intervals, is 95 minutes at Plymouth, and 93 minutes at Bristol. But these are greater than the values which this inequality assumes at any other places. It is 90^m at London and at Sheerness; 86^m at Liverpool and Howth; 84^m at Leith; 83^m at Portsmouth and at Pembroke; 82^m at Ramsgate; and only 80^m at Brest. In each of these cases the value is determined from observations so numerous as to be certain within a minute or two. We see, therefore, how different the mass of the moon would be found to be by calculations from the tide observations of different places.

The comparison of the semimenstrual inequality of height at different places gives a similar result. In this case, however, we must not take the actual amount of the inequality, but its proportion to the mean tide. At Portsmouth the mean range of the tide is $12\frac{1}{2}$ feet; the total semimenstrual inequality, or difference of the height of high water at neap and spring tides, is $2\frac{1}{2}$ feet; that is, only $\frac{1}{5}$ of the mean tide. But at Plymouth, where the mean tide is also $12\frac{1}{2}$ feet, the total semimenstrual inequality is 3·4 feet, and thus the fraction is $\frac{1}{3\cdot4}$. And at Bristol, where the mean range of tide is 33 feet, the semimenstrual inequality is 10 feet, or $\frac{1}{3\cdot3}$ of the mean tide.

The parallax corrections of time are as follows (for 60′), in minutes and decimals:

	m	m	h
London	− 4·6	− 8·8 . s, 2 φ	− 14;
Liverpool ...	2·5	− 7·3 . s, 2 φ	− 13;
Plymouth ...	7·2	− 9·1 . s, 2 φ	− 14;
Bristol, 1834,	10·7	− 12·3 . s, 2 φ	− 14;
1835,	8·5	− 10·5 . s, 2 φ	− 14;
1836,	6	− 9· . s, 2 φ	− 13;
1837,	11	− 12· . s, 2 φ	− 13.

The parallax corrections of height are as follows, in decimals of feet (for 60′):

		h
London	·75 + ·23 . s, 2 φ	− 18;
Liverpool ...	1·35 + ·09 . s, 2 φ	− 18;
Plymouth ...	·57 + ·09 . s, 2 φ	− 14;
Bristol, 1834,	2·35 + ·65 . s, 2 φ	− 15;
1835,	2·1 + ·60 . s, 2 φ	− 16;
1836,	1·85 + ·65 . s, 2 φ	− 14;
1837,	2·1 + ·50 . s, 2 φ	− 13½.

The constant part, which is here much the most considerable, is nearly in the proportion of the mean tide.

The declination corrections of time are (for 7°),

	m	m	h
Plymouth ...	3·5	− 6·5 . s, 2 φ	− 12⅓;
Bristol, 1834,	− 0·5	− 5·5 . s, 2 φ	− 12½;
1835,	1·5	− 7·5 . s, 2 φ	− 13¼;
1836,	1·5	− 7·5 . s, 2 φ	− 13¾;
1837,	3·3	− 7·7 . s, 2 φ	− 12⅔.

The declination corrections of height are (for 7°),

		h
Plymouth ...	·28 + ·15 . s, 2 φ	− 9;
Bristol, 1834,	·9 + ·6 . s, 2 φ	− 14;
1835,	1·1 + ·3 . s, 2 φ	− 15½;
1836,	1·5 + ·7 . s, 2 φ	− 15⅔;
1837,	1·3 + ·5 . s, 2 φ	− 15½.

I do not compare with these the declination corrections for London and Liverpool, because they are calculated from a different mean declination (15°), and therefore a difference might not prove a real discrepancy. For the same reason the Bristol results for different years are not to be expected to agree exactly.

The mean declination of the moon, about which the effects balance each other in an entire revolution of her nodes, appears by calculation to be about 16½°. It is this declination which should, in the Tables, have the correction 0; and the semimenstrual inequality of all years must be taken for this mean declination, in order to

make the correction tables general. I have not yet reduced the Bristol corrections to this basis, having a hope of still further improving the declination correction; and also of obtaining a solar correction from the observations. These tasks, however, must be reserved for a future occasion, if I am able to execute them at all. I hope the present attempt to solve some of the most obvious and important questions of Tidology will not be without its value.

Trinity College, Cambridge,
June 5, 1838.

XI. *Description of a new Tide-Gauge, constructed by Mr.* T. G. Bunt, *and erected on the eastern bank of the river Avon, in front of the Hotwell House, Bristol,* 1837. *Communicated by the Rev.* William Whewell, *M.A. F.R.S. &c.*

Received March 15,—Read March 22, 1838.

THE principal parts of this machine are:

A, an eight-day clock, which turns

B, a vertical cylinder, revolving once in twenty-four hours:

C, a wheel, to which an alternate motion is communicated by

D, a float rising and falling with the tide, and connected with C by the wire E, passing over the pulley F, and kept constantly strained by the counterpoise G:

H, a small drum on the same axis with C, which, by a suspending wire, communicates one-eighteenth of the vertical motion of the float to

I, a bar carrying a pencil, K, which marks a curve on the cylinder B, so as to show the time and height of high water.

The above will convey a general notion of the machine and its use; but it will be necessary to enter more into detail.

a (figs. 1, 2 and 3) is a strong oak frame secured to the quay wall; b is a mahogany frame resting on the arms c c, which are supported by the pillars d d; e is a bracket resting on a pillar f, and sustaining the cylinder B. The lower pivot of the cylinder has its bearing in a small cylindrical plug *b* (fig. 5.), which may be lowered by a screw and a winch, as seen in the figure, so as to disengage, first the upper pivot, and afterwards the lower one, when the cylinder may be drawn out of its place, being made to slide forwards horizontally on the two cross pieces *a a*.

The pencil K, fig. 4, is inserted into a tube soldered to the brass bar *d*. This bar moves without shake, on a long axis *e*, so as not to require side friction to guide it, while it is pressed forwards against the cylinder by the weight and bent lever L; *f* is a small bolt for holding back the pencil, when necessary, by thrusting it into the stud *g*; *h* is a nut and screw for adjusting the height of the pencil to the scale. The pencil is guided vertically by a mahogany bar N, fig. 3, having a groove in its edge for receiving the ivory wheel *o*, which is pressed gently against N by the brass rod P, slightly curved towards the pencil, to produce the requisite pressure. Q Q are two other brass rods (or tubes) for guiding the lower end of the pencil bar I, by means of the cross guide R, fitted loosely on the rods. The guide N is adjusted, or may be drawn out, by means of a notch in the board T, which supports it, the top of the bar being held by a tenon. The pillar d is represented as broken off, for the purpose of

showing the guides N and Q. The outer support S, fig. 2, of the axis of the wheel C, is also removed.

The cylinder, which is 24 inches in length and 48 in circumference, is composed of mahogany staves 2 inches in width and 1 inch in thickness. It has two heads and a diaphragm, also of mahogany, into which every stave is screwed by a screw at each end, and another in the middle. A strong iron axis passes through the whole length of the cylinder, which revolves on a rounded steel pivot screwed into the lower end of the axis. The surface is a white enamel, fit for receiving the mark of a pencil. On the top is screwed a brass wheel, equal in diameter to the cylinder, and divided into 360 teeth. A two-hour pinion (*p*) of thirty teeth, at the bottom of the clock face, causes this wheel to perform one revolution in twenty-four hours. This pinion, together with the rest of the train, is seen in fig. 6.

The clock is of a superior kind, and larger size than ordinary, having a fir pendulum rod and a dead-beat escapement. It goes exceedingly well, and appears to suffer no derangement in the regularity of its motion from the incumbrance of the cylinder. This, however, is very trifling, a weight of half an ounce being sufficient to overcome it. The pallets may be detached from the train by lifting a latch behind the clock, and drawing them backwards. This arrangement is required from its being necessary to *fix* the hands so that they cannot, as in other clocks, be made to slide without carrying the train along with them. The pinion which drives the cylinder may be detached from it by pulling it forwards, so as to permit the cylinder to be turned freely round.

Although the workmanship of the clock is good, yet, from inequalities in the teeth of the wheels, the cylinder is not moved round through exactly equal spaces in equal times. To prevent error from this cause, the hours and minutes marked round the top of the cylinder, were divided out in the following manner. The cylinder being connected with the clock, but the pallets and pendulum detached from it, the hands, which now move rapidly forward, are brought to the instant of $0^h\ 0^m$, where they are held fast while a vertical line is drawn on the cylinder, by the pencil moving between the guides. The hands are then moved forwards 20 minutes at a time, and a line drawn on the cylinder at each of these periods, until the whole circumference has been divided into 72 spaces. These are afterwards subdivided to every 5 minutes. Another necessary precaution is, to *mark* a tooth of the large wheel on the cylinder, and the *two corresponding teeth* of the small wheel which turns it, and in the same manner to mark all the wheels in the train, so that whenever the clock is taken to pieces to be cleaned or repaired, the wheels may be put together again, with the teeth in exactly the same relative positions as before. By this means, a source of considerable error is entirely obviated.

The scale of height may be obtained, by marking the outside of the trunk containing the float into feet, and watching the ascent of the surface of the tide until it reaches one of the lower marks, when a mark must be instantly made with the pencil

on the cylinder. Successive marks, as the tide and pencil continue to ascend, at length complete the scale. A better and more expeditious method is by accurately measuring and dividing the circumference of the large float wheel, and then marking the cylinder with the pencil as it is wound up, by moving the wheel round and bringing its divisions successively up to a fixed index.

The pencil curves may be either received on the enamelled surface of the cylinder, or they may be taken off on paper, very correctly, in the following manner. The sheet having been wrapped round the cylinder, and expanded by rubbing it all over with a wet sponge, is then pasted together at the ends, and secured in its place, either with drawing pins, or with elastic bands, as shown in the drawing; after which it soon becomes quite tight, and the curves may be taken and read off on the paper with nearly as much accuracy as on the enamel itself, with the additional advantage of their being preserved.

The times and heights are read off by means of the vertical scale M, which is held at the two extremities by notches cut in the frame, and may be pushed into contact with the cylinder. The large wheel being detached from the clock, and a short chord-line drawn under the summit of each of the curves and bisected, the point of bisection is turned round to the edge of the scale, the upper end of which shows the time of high water, amongst the divisions of hours and minutes on the cylinder. The height is read off by noticing where the summit of the curve falls amongst the graduations of the scale. Thus in fig. 2. the curve in contact with the scale indicates the time of high water to be $0^h\ 40^m$ P.M. and the height 32 feet 11 inches.

The float is of pine, well saturated with oil, and has hitherto preserved sufficient buoyancy. It is without friction rollers, which it is better to dispense with when its motion is very nearly vertical. The trunk is of cast iron $\frac{5}{8}$ths of an inch in thickness, being in a situation where considerable strength was required. At the back of the trunk is a stout wooden plank, and both together are firmly secured with nuts, and bolts inserted with lead into the quay-wall. The joints are caulked, and the water enters through a moveable shutter in the bottom, by an aperture of about $\frac{1}{100}$th of the sectional area of the trunk. This was found after many trials to be the best size for preventing unsteady motion of the float, which is now almost entirely removed; a rapid undulation being communicated to it when the aperture was made much larger, and a slow but very considerable oscillation when it was much less.

XII. *Remarks on the Theory of the Dispersion of Light, as connected with Polarization. By the Rev.* Baden Powell, *M.A. F.R.S. F.G.S., Savilian Professor of Geometry in the University of Oxford.*

Received May 10,—Read May 31, 1838.

Introductory Observations.

(1.) In the course of four successive papers, I have laid before the Royal Society the comparison of observations of the refractive indices for the standard rays in various media, with the results calculated from the formula of theory, as deduced upon the most improved views of the hypothesis of undulations; the cases discussed including the greatest range of data which experiment has yet furnished.

The degree of accordance thus exhibited between observation and theory, even in the most extreme case, I believe will now be considered sufficient to warrant the conclusion, that the theory at least affords a very satisfactory approximation to the expression and explanation of the actual law of nature; and this in the instance of a class of phenomena which it had long been the reproach of that theory to be supposed incapable of accounting for, if they were not absolutely contradictory to it. At any rate, if, in the instances referred to, the discrepancies should by any be thought still too great, or if it should be contended that other cases may yet arise in which theory may be put to a severer test, yet, with so strong a presumption in its favour, the only fair inference would be, that further examination was required of the principles on which any extension or modification of the theory might be pursued.

(2.) The investigation, then, having advanced thus far, it seemed desirable, as a sequel to my former papers, to devote the present to some remarks connected with the *theory* which has been thus applied.

The facts of interference, on which the undulatory theory was originally based by Dr. Young, obliged us to adopt *some* idea of an *alternating* motion, as well as a motion of *translation*, in our conception of light. And this, with all the accessions it has received, especially from the investigations of Fresnel, has at the present day been connected by the labours of M. Cauchy and others with general dynamical principles, which regulate the propagation of vibratory motions through an elastic medium.

(3.) More precisely, from such dynamical principles certain differential equations of motion have been deduced; the integration of which gives the well-known expression for a wave, involving the relation between the velocity and the wave-length which explains the dispersion. The *direct* and *complete* integration of these forms, effected

by M. Cauchy, and simplified by Mr. Tovey and Mr. Kelland, involves certain conditions, viz. *the evanescence of certain terms*; the interpretation of which implies peculiar views of the constitution of the ether.

Whether a *direct* and *complete* solution, *without* the introduction of these conditions, can be attained, does not appear. But Mr. Tovey has been the first (as far as I am aware) to point out (in one of his valuable series of papers in the London and Edinburgh Journal of Science) that, *without* these conditions, a certain form of the wave-function *is a particular solution* of the equations; and this form is precisely that expressing *elliptically polarized light*.

(4.) Upon a careful examination of that most important and ingenious investigation, it appeared to me that there were one or two questions connected with the subject still requiring to be cleared up.

If the *absence* of the conditions in question be *essential* to the case of elliptically and circularly polarized light, it follows that *all the preceding investigations* (which depend on the fulfilment of those conditions) *are applicable only to unpolarized and plane-polarized light*, and consequently the *general* integration is limited in a most material part of its application; a defect which is only remedied by the supplementary investigation of Mr. Tovey, in which, for this case, a *particular* solution is assigned.

It seemed, then, necessary to show explicitly that the non-fulfilment of the conditions, that is, the *non-evanescence* of the terms in question, is *essential* for elliptically polarized light, as their *evanescence* is for common light; and thus to exhibit distinctly the relation between the cases of elliptically polarized, and those of plane-polarized and unpolarized light: and again, to remove, if possible, the obscurity and discrepancy of opinion in which the physical interpretation of those conditions, with regard to the supposed constitution of the etherial medium, appeared to be involved.

With this object, it seemed necessary to take up the subject from its first principles; and, in pursuing the inquiry, I gladly acknowledge the valuable suggestions I have derived in the course of some correspondence with Mr. Lubbock. I have been thus led to combine the elucidation of the points just referred to, in a connected view with preceding results, by a simplified method; and to trace clearly the relation between the changes in the conditions supposed with regard to the ether, the resulting state of polarization in the ray, and the expression for the dispersion.

Analytical Investigation.

(5.) For the purpose of the ensuing investigation it will be necessary to premise a brief statement of the formulas adopted from the undulatory theory. On the ordinary principles of that theory, the form

$$u = \Sigma \{a \sin (n t - k x)\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

is the sum of a number of functions, each expressing a single wave, whose aggregate constitutes a ray of light; (u) is the displacement of the vibrating molecule of ether;

(α) the amplitude of vibration; (t) the time from the beginning of the disturbance; (x) the distance along the ray from the molecule first agitated; while $\left(\frac{n}{k}\right)$ is the velocity of propagation, which further involves the relation

$$\frac{1}{\mu} = \frac{n}{k} \quad \text{and } k = \frac{2\pi}{\lambda}, \quad \ldots\ldots\ldots\ldots \quad (2.)$$

where (μ) is the refractive index, and (λ) the wave-length in the medium.

(6.) The investigations of FRESNEL require us to suppose the vibrations performed in planes at right angles to the direction of the ray: or, in general, referring the whole motions to three rectangular axes x, y, z, of which we may suppose x to coincide with the direction of the ray, and naming the displacements in each of those directions respectively ξ, η, ζ, we must suppose $\xi = 0$, while each of the others are functions of x and t of the form above, viz.

$$\left.\begin{aligned} \eta &= \Sigma\,\{\alpha \sin (n\,t - k\,x)\} \\ \zeta &= \Sigma\,\{\beta \sin (n\,t - k\,x)\} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (3.)$$

In polarized light the amplitudes α β are in FRESNEL's notation

$$\alpha = \cos i \quad \text{and } \beta = \sin i,$$

where i is the angle formed by the direction of the vibration with that of polarization; whence also

$$\alpha^2 + \beta^2 = 1. \quad \ldots\ldots\ldots\ldots \quad (4.)$$

In plane polarized light either $i = 0$, or $i = \frac{\pi}{2}$; hence one of the expressions (3.) disappears.

For elliptically polarized light both are retained; but one vibration is retarded by a quantity (b), or we must take

$$\left.\begin{aligned} \eta &= \Sigma\,\{\alpha \sin (n\,t - k\,x)\} \\ \zeta &= \Sigma\,\{\beta \sin (n\,t - k\,x + b)\}, \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (5.)$$

expressions which will easily be found to give, on substitution of the value of η in that of ζ, the equation to the ellipse described by the etherial molecules, which becomes that of a circle if $b = \frac{\pi}{2}$ and $\alpha = \beta$. If $\beta = 0$ the formulas are reduced to those for common light, and α and β are not restricted by the condition (4.).

(7.) From (2.) we have $\frac{1}{\mu} = \frac{n}{2\pi}\lambda$; $\quad \ldots\ldots\ldots\ldots \quad (6.)$

hence we may observe; 1st. If $\frac{n}{k}$ be constant there is no dispersion. In this case it is easily found that the formulas (3.) are solutions of the well-known equation of vibratory motion,

$$\frac{d^2 u}{d\,t^2} = -\frac{n^2}{k^2}\frac{d^2 u}{d\,x^2}.$$

2ndly. If n be an independent constant these expressions give the index in the simple inverse ratio of the wave-length, which is manifestly not the law of the unequal refrangibility of the primary rays, as appears at once from what is termed the irrationality of dispersion. But the application of the formulas to the ordinary phenomena is independent of this, and will be equally valid if μ be dependent on λ, precisely or very nearly in the relation expressed by the formula which has been compared with experiment in my previous papers, and which thus explains the unequal refrangibility.

Now it has been shown by the distinguished mathematicians already mentioned, that, under certain conditions, the expressions (3.) can be derived from equations of motion for the vibrations of an elastic medium on dynamical principles, which also involve the desired relation between μ and λ. It will be necessary to state and explain these equations, which may be most shortly done as follows:

(8.) Let the coordinates in space of any molecules m m' be respectively

$$\left.\begin{array}{llll} m \ldots\ldots & x & y & z \\ m' \ldots\ldots & x+\Delta x & y+\Delta y & z+\Delta z \end{array}\right\} \quad \ldots\ldots\ldots \quad (7.)$$

and the distance between them

$$r = \sqrt{(\Delta x^2 + \Delta y^2 + \Delta z^2)} \quad \ldots\ldots\ldots\ldots \quad (8.)$$

Let the force which maintains the system of molecules as an elastic medium be any function of the distance as $f(r)$: then it will be easily seen that we have for the forces in the direction of the three axes, in *equilibrium*,

$$\left.\begin{array}{l} \Sigma f(r)\frac{\Delta x}{r} = 0 \\ \Sigma f(r)\frac{\Delta y}{r} = 0 \\ \Sigma f(r)\frac{\Delta z}{r} = 0 \end{array}\right\} \quad \ldots\ldots\ldots \quad (9.)$$

Let the system be *disturbed*, and after a time (t) let the displacements in the direction of the three axes be respectively

$$\left.\begin{array}{llll} m \ldots\ldots & \xi & \eta & \zeta \\ m' \ldots\ldots & \xi+\Delta\xi & \eta+\Delta\eta & \zeta+\Delta\zeta \end{array}\right\} \quad \ldots\ldots\ldots \quad (10.)$$

also the distance becomes

$$r + \Delta r = \sqrt{\{(\Delta x + \Delta\xi)^2 + (\Delta y + \Delta\eta)^2 + (\Delta z + \Delta\zeta)^2\}} \quad \ldots\ldots \quad (11.)$$

In this condition it will be easily seen that we have for the forces in the direction of the three axes,

$$\left.\begin{array}{l} \frac{d^2\xi}{dt^2} = \Sigma\left\{f(r+\Delta r)\frac{\Delta x + \Delta\xi}{r+\Delta r}\right\} \\ \frac{d^2\eta}{dt^2} = \Sigma\left\{f(r+\Delta r)\frac{\Delta y + \Delta\eta}{r+\Delta r}\right\} \\ \frac{d^2\zeta}{dt^2} = \Sigma\left\{f(r+\Delta r)\frac{\Delta z + \Delta\zeta}{r+\Delta r}\right\} \end{array}\right\} \quad \ldots\ldots\ldots \quad (12.)$$

On expanding the value of $\frac{f(r+\Delta r)}{r+\Delta r}$, neglecting the squares, substituting in the above equations (12.), observing that they will involve the expressions (9.) for equilibrium, which vanish, and writing for abridgement,

$$\frac{f(r+\Delta r)}{r+\Delta r} = \varphi(r) = \frac{f(r)}{r} + \psi(r)\, r\, \Delta r.$$

while we have

$$r\,\Delta r = \Delta x\,\Delta\xi + \Delta y\,\Delta\eta + \Delta z\,\Delta\zeta;$$

these equations of motion (12.) will ultimately become

$$\left.\begin{aligned} \frac{d^2\xi}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\xi + \psi(r)\,\Delta x\,(\Delta x\,\Delta\xi + \Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)\right\} \\ \frac{d^2\eta}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\eta + \psi(r)\,\Delta y\,(\Delta x\,\Delta\xi + \Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)\right\} \\ \frac{d^2\zeta}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\zeta + \psi(r)\,\Delta z\,(\Delta x\,\Delta\xi + \Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)\right\}. \end{aligned}\right\} \quad . \quad (13.)$$

These equations form the common basis of all the investigations of the subject as originally pursued by MM. Navier and Cauchy. They apply generally to all elastic media, or systems of molecules, affected only by their mutual attractions and repulsions, and slightly disturbed.

(9.) In applying this hypothesis to the case of *light*, agreeably to what has been already observed (§. 6.), we have

$$\Delta\xi = 0 \quad \text{and} \quad \frac{d^2\xi}{dt^2} = 0,$$

so that the equations (13.) are reduced to

$$\left.\begin{aligned} \frac{d^2\eta}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\eta + \psi(r)\,\Delta y\,[\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta]\right\} \\ \frac{d^2\zeta}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\zeta + \psi(r)\,\Delta z\,[\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta]\right\} \end{aligned}\right\} \quad . \quad . \quad . \quad (14.)$$

The *general direct* integration of these forms, and even of the preceding (13.), has been effected by the writers already referred to; *but only upon the suppositions*

$$\Sigma\,\{\Delta y\,\Delta z\} = 0, \quad \text{and } \Sigma\,\{\sin k\,\Delta x\} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (15.)$$

the import of which will be considered in the sequel. Mr. Tovey by a skilful analysis gives a *particular solution* corresponding to the case of elliptically polarized light, *without introducing those conditions*. The investigation, however, appears susceptible of being simplified; and we shall here pursue a method at once attaining this object, and enabling us more clearly to trace out some interesting results, as follows:

Taking the finite differences of the expressions (5.), we have

$$\Delta\eta = \alpha\left[2\sin^2\left(\frac{k\,\Delta x}{2}\right)\sin(n\,t - k\,x) - \sin k\,\Delta x\cos(n\,t - k\,x)\right] \quad . \quad . \quad (16.)$$

$$\Delta\zeta = \left\{\begin{array}{l} \beta\cos b\left[2\sin^2\left(\frac{k\Delta x}{2}\right)\sin(nt-kx) - \sin k\Delta x\cos(nt-kx)\right] \\ +\beta\sin b\left[\sin k\Delta x\sin(nt-kx) - 2\sin^2\left(\frac{k\Delta x}{2}\right)\cos(nt-kx)\right]. \end{array}\right\} \quad (17.)$$

Also, differentiating them, we find,

$$\frac{d^2\eta}{dt^2} = -n^2\alpha\sin(nt-kx) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

$$\frac{d^2\zeta}{dt^2} = -n^2\beta\cos b\sin(nt-kx) - n^2\beta\sin b\cos(nt-kx) \quad . \quad . \quad . \quad (19.)$$

On arranging the terms in (14.) we may write for abridgement,

$$\left.\begin{array}{l} p = \varphi(r) + \psi(r)\Delta y^2 \\ p' = \varphi(r) + \psi(r)\Delta z^2 \\ q = \psi(r)\Delta y\Delta z \\ 2\theta = k\Delta x \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20.)$$

and those forms (14.) will thus become,

$$\frac{d^2\eta}{dt^2} = \Sigma\left[p\Delta\eta + q\Delta\zeta\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21.)$$

$$\frac{d^2\zeta}{dt^2} = \Sigma\left[p'\Delta\zeta + q\Delta\eta\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22.)$$

On substituting in these forms the values of $\Delta\eta$ and $\Delta\zeta$ above, viz. (16.) (17.), we shall have,

$$\frac{d^2\eta}{dt^2} = \Sigma\left\{\begin{array}{l} \left.\begin{array}{l} q\{\beta\sin b\} \\ +q\{\beta\cos b\} \\ +p\{\alpha \end{array}\right\} \begin{array}{l} \sin 2\theta \\ 2\sin^2\theta \end{array} \Bigg\} \sin(nt-kx) \\ \left.\begin{array}{l} -p\{\alpha \\ -q\{\beta\cos b\} \\ -q\{\beta\sin b\} \end{array}\right\} \begin{array}{l} \sin 2\theta \\ 2\sin^2\theta \end{array} \Bigg\} \cos(nt-kx) \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (23.)$$

$$\frac{d^2\zeta}{dt^2} = \Sigma\left\{\begin{array}{l} \left.\begin{array}{l} p'\{\beta\sin b\} \\ +p'\{\beta\cos b\} \\ +q\{\alpha \end{array}\right\} \begin{array}{l} \sin 2\theta \\ 2\sin^2\theta \end{array} \Bigg\} \sin(nt-kx) \\ \left.\begin{array}{l} -q\{\alpha \\ -p'\{\beta\cos b\} \\ -p'\{\beta\sin b\} \end{array}\right\} \begin{array}{l} \sin 2\theta \\ 2\sin^2\theta \end{array} \Bigg\} \cos(nt-kx) \end{array}\right\} \quad . \quad . \quad . \quad . \quad (24.)$$

On comparing these expressions with those for the same functions (18.) (19.) which must be identical, and equating in each the terms which form the coefficients respectively of $\sin(nt-kx)$ and of $\cos(nt-kx)$ (since they must hold good for all

values of those variable terms), we shall have the following equations :

$$-n^2\alpha = \Sigma\left\{\begin{matrix}+q\{\beta\sin b\} \\ +q\{\beta\cos b\} \\ +p\{\alpha\end{matrix}\right\}\begin{matrix}\sin 2\theta \\ 2\sin^2\theta\end{matrix}\Bigg\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (25.)$$

$$0 = \Sigma\left\{\begin{matrix}-p\{\alpha \\ -q\{\beta\cos b \\ -q\{\beta\sin b\}\end{matrix}\right\}\begin{matrix}\sin 2\theta \\ 2\sin^2\theta\end{matrix}\Bigg\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (26.)$$

$$-n^2\beta\cos b = \Sigma\left\{\begin{matrix}+p'\{\beta\sin b\} \\ +p'\{\beta\cos b\} \\ +q\{\alpha\end{matrix}\right\}\begin{matrix}\sin 2\theta \\ 2\sin^2\theta\end{matrix}\Bigg\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (27.)$$

$$-n^2\beta\sin b = \Sigma\left\{\begin{matrix}-q\{\alpha \\ -p'\{\beta\cos b \\ -p'\{\beta\sin b\}\end{matrix}\right\}\begin{matrix}\sin 2\theta \\ 2\sin^2\theta\end{matrix}\Bigg\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (28.)$$

From the two last forms (27.) (28.) we obtain by multiplication and addition,

$$-n^2\beta = \Sigma\left\{\begin{matrix}p'\{\beta\cos 2b \\ q\{\alpha\cos b \\ -q\{\alpha\sin b\end{matrix}\right\}\begin{matrix}2\sin^2\theta \\ \sin 2\theta\end{matrix}\Bigg\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (29.)$$

And in like manner from (25.) and (29.) we deduce

$$-n^2(\alpha^2+\beta^2) = \Sigma\left\{\begin{matrix}p'\{\beta^2\cos 2b \\ +p\{\alpha^2 \\ +q\{2\alpha\beta\cos b\end{matrix}\right\}2\sin^2\theta\Bigg\} \quad . \quad . \quad . \quad . \quad (30.)$$

In the case of elliptically polarized light we have

$$\alpha^2+\beta^2 = 1\,;$$

and it is thus obvious that *when the quantities involved give the value of* n^2 *as here expressed*, viz.

$$-n^2 = \Sigma\left\{\begin{matrix}p'\{\beta^2\cos 2b\} \\ +p\{\alpha^2\} \\ +q\{2\alpha\beta\cos b\}\end{matrix}\right\}2\sin^2\theta\Bigg\} \quad . \quad . \quad . \quad . \quad (31.)$$

and when this value is substituted in (18.) (19.), *and consequently in* (6.), *the formula* (5.) *for elliptically polarized light is a particular solution of the differential equations of motion of the system of molecules* (14.).

Deductions and Remarks.

(10.) It is evident that in (31.) expressing the whole coefficient by a single letter, we have the abridged form

$$n^2 = \Sigma\{H'^2\sin^2\theta\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32.)$$

which, on dividing by k^2, restoring the value of θ (20.) and of k (2.), will give for the velocity $\left(\text{or } \frac{n}{k}\right)$ (on multiplying both numerator and denominator by $\left(\frac{\Delta x}{2}\right)$ on the second side of the equation, and including all the coefficients in a new single term,) the formula

$$\frac{1}{\mu^2} = \Sigma\left\{H^2 \frac{\sin^2\left(\frac{\pi \Delta x}{\lambda}\right)}{\left(\frac{\pi \Delta x}{\lambda}\right)^2}\right\}, \quad \ldots \ldots \ldots \quad (33.)$$

the same which has been deduced for unpolarized and plane polarized light.

From this formula, on the supposition of a common coefficient, I made my approximate computations in Nos. I. and II. of my former Researches. On expanding the sine and dividing by the arc there results a series of even powers of λ, which, in a generalized form, is that from which Sir W. R. HAMILTON deduced his elegant formula for interpolation, by which I performed my calculations in No. III. From a similar series, also, M. CAUCHY has deduced the elaborate method of calculation which he has applied to FRAUNHOFER'S indices in his *Nouveaux Exercices de Mathématiques*, and also in his lithographed memoir on Interpolation: whilst the same series obtained by Mr. KELLAND, and modified by the introduction of the value of λ *in vacuo* (that term being in the above formula the wave-length *in the medium*), or

$$P = \frac{1}{\mu^2} + \left(\frac{\mu}{\lambda}\right)^2 Q - \left(\frac{\mu}{\lambda}\right)^4 L \quad \ldots \ldots \ldots \quad (34.)$$

is the formula which I employed and explained in No. IV. Precisely equivalent results are also deduced by Mr. TOVEY.

(11.) The term on which the dispersion depends, viz.

$$\frac{\sin\left(\frac{\pi \Delta x}{\lambda}\right)}{\left(\frac{\pi \Delta x}{\lambda}\right)}, \quad \ldots \ldots \ldots \quad (35.)$$

manifestly approaches to unity as Δx diminishes in comparison with λ, the application of which was originally pointed out by Mr. AIRY. The dispersion is insensible in free space, and large in dense media. Hence Mr. KELLAND has inferred that the ether is in a state of *greater* density in free space, and *less* in dense media. We shall recur to this point in the sequel.

(12.) Experiment does not show that the state of a ray as to polarization, produces any difference in the magnitude of its refractive index. Hence it follows that in the formula (30.) *the values of* n^2 *must be constant for all values of* b, *as well as of* α *and* β. Hence for any particular value of b the several sums of terms involved must be supposed to vary in magnitude, so that, joined with the condition (4.), the whole expression shall always remain constant, and equal to that in the case where b vanishes, and where α and β are no longer restricted to the condition (4.).

The examination of the cases thus arising, is closely connected with the question of the evanescence of the terms before alluded to (15.). These relations therefore we shall now proceed to trace. But it will be desirable first to point out more clearly the nature and meaning of those conditions.

(18.) It is evident from the value of r (8.) that it does not change its sign along with a change of sign in Δx, Δy, or Δz; the same is true therefore of $\varphi(r)$ and $\psi(r)$. It is also evident that Δy^2, Δz^2, do not change the signs. But the product $\Delta y \Delta z$, will change signs as each value of Δy corresponds to a negative and a positive value of Δz.

Hence the terms

$$\psi(r)\sin k\Delta x$$
$$\psi(r)\Delta y^2 \sin k\Delta x$$
$$\psi(r)\Delta z^2 \sin k\Delta x$$
$$\psi(r)\Delta y \Delta z \sin k\Delta x$$
$$\psi(r)\Delta y \Delta z \sin^2\left(\frac{k\Delta x}{2}\right)$$

will have values with opposite signs. But,

$$\psi(r)\sin^2\left(\frac{k\Delta x}{2}\right)$$
$$\psi(r)\Delta y^2 \sin^2\left(\frac{k\Delta x}{2}\right)$$
$$\psi(r)\Delta z^2 \sin^2\left(\frac{k\Delta x}{2}\right)$$

will have all their values with the same sign. If then we consider the sums of a number of such terms respectively, those of the first set may become $= 0$ if the positive and negative sums be equal. In those of the second set this cannot happen.

Hence resuming our abridged notation we may have either

$$\left.\begin{aligned}\Sigma(p\sin 2\theta) &= 0\\ \Sigma(p'\sin 2\theta) &= 0\\ \Sigma(q\sin 2\theta) &= 0\end{aligned}\right\} \quad . \quad . \quad . \quad (36.)$$

$$\Sigma(q\sin^2\theta) = 0 \quad . \quad . \quad . \quad . \quad (37.)$$

or

$$\left.\begin{aligned}\Sigma(p\sin 2\theta) &= s\\ \Sigma(p'\sin 2\theta) &= s_i\\ \Sigma(q\sin 2\theta) &= s_{ii}\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (38.)$$

$$\Sigma(q\sin^2\theta) = s_{iii} \quad . \quad . \quad . \quad . \quad . \quad (39.)$$

but always

$$\left.\begin{aligned}\Sigma(p\sin^2\theta) &= s'\\ \Sigma(p'\sin^2\theta) &= s''\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (40.)$$

The conditions which give these values respectively, are dependent on the general supposition relative to the arrangement of the molecules. If the distribution of the molecules in space be uniform, the values (36.) (37.) obtain. If not uniform, the

values (38.) (39.) may obtain; but in particular cases, or for particular directions of the ray, the greater number of terms of one sign may compensate for the greater magnitudes of those of the other; or the conditions (36.) (37.) may hold good.

(14.) We now resume the consideration of the formulas before deduced.

From (28.) we have

$$\sin b = \frac{\alpha \Sigma (q \sin 2\theta) + \beta \cos b \Sigma (p' \sin 2\theta)}{n^2 \beta - 2\beta \Sigma (p' \sin^2 \theta)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (41.)$$

Now if the conditions (36.) obtain, that is, if we have

$$\Sigma (q \sin 2\theta) = 0, \text{ and } \Sigma (p' \sin 2\theta) = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42.)$$

then (41.) will give

$$\sin b = 0; \text{ or } b = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43.)$$

But if the polarization be elliptical, b must have a finite value; or in this case, consequently, the conditions (38.) must obtain; or *the non-evanescence of those terms is essential to the investigation for elliptically polarized light.*

(15.) If the polarization be *circular*, we have

$$\alpha = \beta, \text{ and } b = \frac{\pi}{2} \cos b = 0, \sin b = 1;$$

thus the form (26.) becomes

$$0 = \left\{ \begin{matrix} \Sigma (p \sin 2\theta) \\ -2 \Sigma (q \sin^2 \theta). \end{matrix} \right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45.)$$

But the condition of *the non-evanescence of the terms holds good* for the same reason as in the last case. . . . . (46.)

(16.) For plane polarized light let $\beta = 0$, then the form (26.) becomes

$$0 = \Sigma (p \sin 2\theta); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (47.)$$

or for plane polarization it is essential to suppose the terms evanescent, or the conditions (36.) (37.) *to be fulfilled.* . . . . . (48.)

(17.) For unpolarized light we have

$$b = 0 \quad \sin b = 0 \quad \cos b = 1;$$

but α and β are arbitrary. Thus the formula (26.) becomes

$$0 = \left\{ \begin{matrix} \alpha \Sigma (p \sin 2\theta) \\ +\beta \Sigma (q \sin 2\theta). \end{matrix} \right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (49.)$$

But since this must be true for all values of α and β which are independent of p, q and θ, it follows that each term separately must be $= 0$; *or for unpolarized light it is essential to suppose the terms evanescent, or the conditions* (36.) (37.) *to be fulfilled.* . . . . . (50.)

(18.) We here take the axis (x) as the direction of the ray, which consequently may

have any position whatever in respect to particular lines of direction which may subsist among the molecules of the etherial medium.

Now, supposing the arrangement of the molecules to be perfectly uniform, since the direction of the ray (x) must pass through m, it follows that the sums of the distances of all those on each side of the line x, whether in the plane of y or of z, will be equal, or the hypothesis (36.) (37.), or (48.) (50.) exactly fulfilled, *for all positions of* x in the medium.

If the arrangement be *not* uniform, the same hypothesis in general may be *approximately* fulfilled in proportion as we suppose a *greater number of such molecules taken into account*, or the summation extended to a greater number of terms, that is, a greater number of molecules *within the sphere of the influence of the propagation of motion* from the molecule first agitated. But if we suppose any portion of the ether be so circumstanced that the sphere of influence is more limited, then the conditions (36.) (37.) will not be fulfilled even approximately, or the hypothesis (38.) (39.) will obtain; though they would approach more or less towards fulfilment in different positions of the ray. The diminution of the sphere of influence may arise either from a decrease in the force, or an increase in the distances of the molecules.

(19.) Here also we may remark, in connexion with what was observed before (§. 11.), that at the bounding surface of vacuum and a medium, or generally of two media of different densities, we can hardly suppose the change of density in the ether to take place abruptly; but must, from all analogy, imagine a thin stratum on either side, within which there is a gradual alteration in the arrangement of the molecules, and this more considerable in proportion to the difference of refractive powers of the media.

Thus, even supposing the molecules uniformly distributed in the two media or portions of space, it is evident that within this stratum they will not be uniformly arranged. And hence, though on either side the conditions (36.) (37.) should be fulfilled, *within the stratum* the conditions (38.) (39.) would take effect.

Conclusion.

(20.) The general conclusions which I conceive have been obtained are as follows:

When light is elliptically or circularly polarized, that is, when one of the two component vibrations is retarded behind the other, then, in the differential equations of motion, the opposite terms do not destroy each other in the summation; that is, the arrangement of the molecules is not uniform.

When light is plane-polarized, or unpolarized, that is, when there is no retardation, or the phases of the component vibrations are simultaneous, then the opposite sums destroy each other; that is, either the arrangement is uniform, or the sphere of the influence of the force so great, that the conditions are fulfilled very nearly.

Since both kinds of light can be propagated indifferently through ordinary media, it follows that the sphere of influence of the force, or number of molecules taken into

account, does not here depend on the arrangement of the molecules of ether in the medium, but on the retardation of one of the vibrations behind the other, or the absence of it, originally impressed on the ray in the respective cases.

These inferences appear of importance in connexion with the consideration of the causes on which polarization depends, and the laws of elasticity of the ether in crystallized bodies, as also the state of the ether at the bounding surfaces of media of different density, and the changes which may be effected in the state of polarization of a ray: and in what has here been laid down, I trust that the whole subject (without entering into specific controversy) will be found relieved from some degree of difficulty and objection.

Note to Art. 6.

If in equation (5.) $b = \frac{\pi}{2}$ (without supposing $\alpha = \beta$) the vibration resulting is still elliptical. In this case the expressions may be put into the form

$$\left.\begin{aligned} \eta &= \Sigma \{\alpha \sin (n t - k x)\} \\ \zeta &= \Sigma \{\beta \cos (n t - k x)\} \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (52.)$$

This form is that adopted by Professor Maccullagh* to express the elliptical vibrations of the two rays in quartz, and by means of which he connects the laws of M. Biot, and the theory of Mr. Airy, with certain differential equations of vibratory motion, provided the quantities α, β, n, and k, are so assumed as to fulfil these conditions, viz. that if A and B be the squares of the velocities of the ordinary and extraordinary rays in common double refraction, in quartz we replace them respectively by

$$\mathrm{A} - k\frac{\beta}{\alpha}\mathrm{C} \quad \text{and} \quad \mathrm{B} - k\frac{\alpha}{\beta}\mathrm{C}$$

where C is a new constant determined from Biot's observations. The differential equations referred to are these:

$$\left.\begin{aligned} \frac{d^2\eta}{dt^2} &= \mathrm{A}\frac{d^2\eta}{dx^2} + \mathrm{C}\frac{d^2\zeta}{dx^2} \\ \frac{d^2\zeta}{dt^2} &= \mathrm{B}\frac{d^2\zeta}{dx^2} - \mathrm{C}\frac{d^2\eta}{dx^2} \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (53.)$$

But on taking the partial differential coefficients of the expressions (52.) it is easily seen (from the particular forms of those functions) that the equations (53.) are reducible to,

$$\left.\begin{aligned} \frac{d^2\eta}{dt^2} &= \left[\mathrm{A} - k\frac{\beta}{\alpha}\mathrm{C}\right]\frac{d^2\eta}{dx^2} \\ \frac{d^2\zeta}{dt^2} &= \left[\mathrm{B} + k\frac{\alpha}{\beta}\mathrm{C}\right]\frac{d^2\zeta}{dx^2} \end{aligned}\right\}$$

which are of the well-known form (§. 7.) for vibratory motion.

* Memoirs of the Royal Irish Academy, 1836.

XIII. *Experimental Researches in Electricity.—Fourteenth Series.* *By* MICHAEL FARADAY, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, Gottingen, Modena, Palermo, &c. &c.*

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§. 20. *Nature of the electric force or forces.* §. 21. *Relation of the electric and magnetic forces.* §. 22. *Note on electrical excitation.*

§. 20. *Nature of the electric force or forces.*

1667. THE theory of induction set forth and illustrated in the three preceding series of experimental researches does not assume anything new as to the nature of the electric force or forces, but only as to their distribution. The effects may depend upon the association of one electric fluid with the particles of matter, as in the theory of FRANKLIN, EPINUS, CAVENDISH, and MOSSOTTI; or they may depend upon the association of two electric fluids, as in the theory of DUFAY and POISSON; or they may not depend upon anything which can properly be called the electric fluid, but on vibrations or other affections of the matter in which they appear. The theory is unaffected by such differences in the mode of viewing the nature of the forces; and though it professes to perform the important office of stating *how* the powers are arranged (at least in inductive phenomena), it does not, as far as I can yet perceive, supply a single experiment which can be considered as a distinguishing test of the truth of these various views.

1668. But, to ascertain how the forces are arranged, to trace them in their various relations to the particles of matter, to determine their general laws, and also the specific differences which occur under these laws, is as important as, if not more so than, to know whether the forces reside in a fluid or not; and with the hope of assisting in this research, I shall offer some further developments, theoretical and experimental, of the conditions under which I suppose the particles of matter are placed when exhibiting inductive phenomena.

1669. The theory assumes that all the *particles*, whether of insulating or conducting matter, are as wholes conductors.

1670. That not being polar in their normal state, they can become so by the influence of neighbouring charged particles, the polar state being developed at the instant, exactly as in an insulated conducting *mass* consisting of many particles.

1671. That the particles when polarized are in a forced state, and tend to return to their normal or natural condition.

1672. That being as wholes conductors, they can readily be charged, either *bodily* or *polarly*.

1673. That particles which being contiguous are in the line of inductive action can communicate or transfer their polar forces one to another *more or less* readily.

1674. That those doing so less readily require the polar forces to be raised to a higher degree before this transference or communication takes place.

1675. That the ready communication of forces between contiguous particles constitutes conduction, and the difficult communication insulation; conductors and insulators being bodies whose particles naturally possess the property of communicating their respective forces easily or with difficulty, and bodies having these differences just as they have differences of any other natural property.

1676. That ordinary induction is the effect resulting from the action of matter charged with excited or free electricity upon insulating matter, tending to produce in it an equal amount of the contrary state.

1677. That it can do this only by polarizing the particles contiguous to it, which perform the same office to the next, and these again to those beyond; and that thus the action is propagated from the excited body to the next conducting mass, and there renders the contrary force evident in consequence of the effect of communication which supervenes in the conducting mass upon the polarization of the particles of that body (1675.).

1678. That therefore induction can only take place through insulators; that induction is insulation, it being the necessary consequence of the state of the particles and the mode in which the influence of electrical forces is transferred or transmitted across such insulating media.

1679. The particles of an insulating dielectric whilst under induction may be compared to a series of small magnetic needles, or more correctly still to a series of small insulated conductors. If the space round a charged globe were filled with a mixture of an insulating dielectric, as oil of turpentine or air, and small globular conductors, as shot, the latter being at a little distance from each other so as to be insulated, then these would in their condition and action exactly resemble what I consider to be the condition and action of the particles of the insulating dielectric itself. If the globe were charged, these little conductors would all be polar; if the globe were discharged, they would all return to their normal state, to be polarized again upon the recharging of the globe. The state developed by induction through such particles on a mass of conducting matter at a distance would be of the contrary kind, and exactly equal in amount to the force in the inductric globe. There would be a lateral diffusion of force (1224. 1297.), because each polarized sphere would be in an active or tense re-

lation to all those contiguous to it, just as one magnet can affect two or more magnetic needles near it, and these again a still greater number beyond them. Hence would result the production of curved lines of inductive force if the inducteous body in such a mixed dielectric were an uninsulated metallic ball (1219. &c.) or other properly shaped mass. Such curved lines are the consequences of the two electric forces arranged as I have assumed them to be: and, that the inductive force can be directed in such curved lines is the strongest proof of the presence of the two powers and the polar condition of the dielectric particles.

1680. I think it is evident that in the case stated, action at a distance can only result through an action of the contiguous conducting particles. There is no reason why the inductric body should polarize or affect *distant* conductors and leave those *near* it, namely the particles of the dielectric, unaffected: and everything in the form of fact and experiment with conducting masses or particles of a sensible size contradicts such a supposition.

1681. A striking character of the electric power is that it is limited and exclusive, and that the two forces being always present are exactly equal in amount. The forces are related in one of two ways, either as in the natural normal condition of an uncharged insulated conductor; or as in the charged state, the latter being a case of induction.

1682. Cases of induction are easily arranged so that the two forces being limited in their direction shall present no phenomena or indications external to the apparatus employed. Thus if a Leyden jar, having its external coating a little higher than the internal, be charged and then its charging ball and rod removed, such jar will present no electrical appearances so long as its outside is uninsulated. The two forces which may be said to be in the coatings, or in the particles of the dielectric contiguous to them, are entirely engaged to each other by induction through the glass; and a carrier ball (1181.) applied either to the inside or outside of the jar will show no signs of electricity. But if the jar be insulated, and the charging ball and rod, in an uncharged state and suspended by an insulating thread of white silk, be restored to their place, then the part projecting above the jar will give electrical indications and charge the carrier, and at the same time the *outside* coating of the jar will be found in the opposite state and inductric towards external surrounding objects.

1683. These are simple consequences of the theory. Whilst the charge of the inner coating could induce only through the glass towards the outer coating, and the latter contained no more of the contrary force than was equivalent to it, no induction external to the jar could be perceived; but when the inner coating was extended by the rod and ball so that it could induce through the air towards external objects, then the tension of the polarized glass molecules would, by their tendency to return to the normal state, fall a little, and a portion of the charge passing to the surface of this new part of the inner conductor, would produce inductive action through the air towards distant objects, whilst at the same time a part of the force in the outer coating previously directed inwards would now be at liberty, and indeed be constrained to

induct outwards through the air, producing in that outer coating what is sometimes called, though I think very improperly, free charge. If a small Leyden jar be converted into that form of apparatus usually known by the name of the electric well, it will illustrate this action very completely.

1684. The terms *free charge* and *dissimulated electricity* convey therefore erroneous notions if they are meant to imply any difference as to the mode or kind of action. The charge upon an insulated conductor in the middle of a room is in the same relation to the walls of that room as the charge upon the inner coating of a Leyden jar is to the outer coating of the same jar. The one is not more *free* or more *dissimulated* than the other, and when sometimes we make electricity appear where it was not evident before, as upon the outside of a charged jar when, after insulating it, we touch the inner coating, it is only because we divert more or less of the inductive force from one direction into another; for not the slightest change is in such circumstances impressed upon the character or action of the force.

1685. Having given this general theoretical view, I will now notice particular points relating to the nature of the assumed electric polarity of the insulating dielectric particles.

1686. The polar state may be considered in common induction as a forced state, the particles tending to return to their normal condition. It may probably be raised to a very high degree by approximation of the inductric and inducteous bodies or by other circumstances; and the phenomena of electrolyzation (861. 1652. 1706) seem to imply that the proportion of power which can thus be accumulated on a single particle is enormous. Hereafter we may be able to compare corpuscular forces, as those of gravity, cohesion, electricity, and chemical affinity, and in some way or other from their effects deduce their relative equivalents; at present we are not able to do so, but there seems no reason to doubt that their electrical, which are at the same time their chemical, forces (891. 918.) will be by far the most energetic.

1687. I do not consider the powers when developed by the polarization as limited to two distinct points or spots on the surface of each particle to be considered as the poles of an axis, but as resident on large portions of that surface, as they are upon the surface of a conductor of sensible size when it is thrown into a polar state. But it is very probable, notwithstanding, that the particles of different bodies may present specific differences in this respect, the powers not being equally diffused though equal in quantity; other circumstances also, as form and quality, giving to each a peculiar polar relation. It is perhaps to the existence of some such differences as these that we may attribute the specific actions of the different dielectrics in relation to discharge (1394. 1508.). Thus with respect to oxygen and nitrogen singular contrasts were presented when spark and brush discharge were made to take place in these gases, as may be seen by reference to the Table in paragraph 1518 of the Thirteenth Series; for

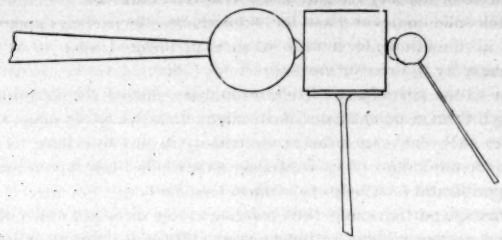
with nitrogen, when the small negative or the large positive ball was rendered inductric, the effects corresponded with those which in oxygen were produced when the small positive or the large negative was rendered inductric.

1688. In such solid bodies as glass, lac, sulphur, &c., the particles appear to be able to polarize in all directions, for a mass when experimented upon so as to ascertain its inductive capacity in three or more directions (1690.), gives no indication of a difference. Now as the particles are fixed in the mass, and as the direction of the induction through them must change with its change relative to the mass, the constant effect indicates that they can polarize electrically in any direction. This accords with the view already taken of each particle as a whole being a conductor (1669.), and, as an experimental fact, helps to confirm that view.

1689. But though particles may thus polarize in *any* direction under the influence of powers which are probably of extreme energy (1686.), it does not follow that each particle may not tend to polarize to a greater degree, or with more facility, in one direction than another; or that different kinds may not have specific differences in this respect, as they have differences of conducting and other powers (1296. 1326. 1395.). I sought with great anxiety for a relation of this nature; and selecting crystalline bodies as those in which all the particles are symmetrically placed, and therefore best fitted to indicate any result which might depend upon variation of the direction of the forces to the direction of the particles in which they were developed, experimented very carefully with them. I was the more strongly stimulated to this inquiry by the beautiful electrical condition of those crystalline bodies tourmaline and boracite, and hoped also to discover a relation between electric polarity and that of crystallization, or even of cohesion itself (1316.). My experiments have not established any connexion of the kind sought for. But as I think it of equal importance to show either that there is or is not such a relation, I shall briefly describe the results.

1690. The form of experiment was as follows. A brass ball 0·73 of an inch in diameter, fixed at the end of a horizontal brass rod, and that at the end of a brass cylinder, was by means of the latter connected with a large Leyden battery (291.) by perfect metallic communications, the object being to keep that ball, by its connexion with the charged battery in an electrified state, very nearly uniform, for half an hour at a time. This was the inductric ball. The inducteous ball was the carrier of the torsion electrometer (1229. 1314.); and the dielectric between them was a cube cut from a crystal, so that two of its faces should be parallel to the optical axis, whilst the other four were perpendicular to it. A small projecting piece of shell-lac was fixed on the inductric ball at that part opposite to the attachment of the brass rod, for the purpose of preventing actual contact between the ball and the crystal cube. A coat of shell-lac was also attached to that side of the carrier ball which was to be towards the cube, being also that side which was furthest from the repelled ball in the electrometer when placed in its position in that instrument. The cube was covered

with a thin coat of shell-lac dissolved in alcohol, to prevent the deposition of damp upon its surface from the air. It was supported upon a small table of shell-lac fixed



on the top of a stem of the same substance, the latter being of sufficient strength to sustain the cube, and yet flexible enough from its length to act as a spring, and allow the cube to bear, when in its place, against the shell-lac on the inductric ball.

1691. Thus it was easy to bring the inducteous ball always to the same distance from the inductric ball, and to uninsulate and insulate it again in its place; and then, after measuring the force in the electrometer (1181.), to return it to its place opposite to the inductric ball for a second observation. Or it was easy by revolving the stand which supported the cube to bring four of its faces in succession towards the inductric ball, and so observe the force when the lines of inductive action (1304.) coincided with, or were transverse to, the direction of the optical axis of the crystal. Generally from twenty to twenty-eight observations were made in succession upon the four vertical faces of a cube, and then an average expression of the inductive force was obtained, and compared with similar averages obtained at other times, every precaution being taken to secure accurate results.

1692. The first cube used was of *rock crystal*; it was 0·7 of an inch in the side. It presented a remarkable and constant difference, the average of not less than 197 observations giving 100 for the specific inductive capacity in the direction coinciding with the optical axis of the cube, whilst 93·59 and 93·31 were the expressions for the two transverse directions.

1693. But with a second cube of rock crystal corresponding results were not obtained. It was 0·77 of an inch in the side. The average of many experiments gave 100 for the specific inductive capacity coinciding with the direction of the optical axis, and 98·6 and 99·92 for the two other directions.

1694. Lord Ashley, whom I have found ever ready to advance the cause of science, obtained for me the loan of three globes of rock crystal belonging to Her Grace the Duchess of Sutherland for the purposes of this investigation. Two had such fissures as to render them unfit for the experiments (1193. 1698.). The third, which was very

superior, gave me no indications of any difference in the inductive force for different directions.

1695. I then used cubes of Iceland spar. One 0·5 of an inch in diameter gave 100 for the axial direction, and 98·66 and 95·74 for the two cross directions. The other, 0·8 of an inch in the side, gave 100 for the axial direction, whilst 101·73 and 101·86 were the numbers for the cross direction.

1696. Besides these differences there were others, which I do not think it needful to state, since the main point is not confirmed. For though the experiments with the first cube raised great expectation, they have not been generalized by those which followed. I have no doubt of the results as to that cube, but they cannot as yet be referred to crystallization. There are in the cube some faintly coloured layers parallel to the optical axis, and the matter which colours them may have an influence; but then the layers are also nearly parallel to a cross direction, and if at all influential should show some effect in that direction, which they did not.

1697. In some of the experiments one half or one part of a cube showed a superiority to another part, and this I could not trace to any charge the different parts had received. It was found indeed that the varnishing of the cubes was sufficient to prevent them receiving any charge, except (in a few experiments) a small degree of the negative state, or that which was contrary to the state of the inductric ball (1564. 1566.).

1698. I think it right to say that, as far as I could perceive, the insulating character of the cubes used was perfect, or at least so nearly perfect as to bear a comparison with shell-lac, glass, &c. As to the cause of the differences, other than regular crystalline structure, there may be several. Thus minute fissures in the crystal insensible to the eye may be so disposed as to produce a sensible electrical difference (1193.). Or the crystallization may be irregular: or the substance may not be quite pure; and if we consider how minute a quantity of matter will alter greatly the conducting power of water, it will seem not unlikely that a little extraneous matter diffused through the whole or part of a cube, may produce effects sufficient to account for all the irregularities of action that have been observed.

1699. An important inquiry regarding the electrical polarity of the particles of an insulating dielectric, is, whether it be the molecules of the particular substance acted on, or the component or ultimate particles, which thus act the part of insulated conducting polarizing portions (1669.).

1700. The conclusion I have arrived at is, that it is the molecules of the substance which polarize as wholes (1347.): and that however complicated the composition of a body may be, all those particles or atoms which are held together by chemical affinity to form one molecule of the resulting body, act as one conducting mass or particle when inductive phenomena and polarization are produced in the substance of which it is a part.

1701. This conclusion is founded on several considerations. Thus if we observe the insulating and conducting power of elements when they are used as dielectrics, we find some, as sulphur, phosphorus, chlorine, iodine, &c., whose particles insulate, and therefore polarize in a high degree; whereas others, as the metals, give scarcely any indication of possessing a sensible proportion of this power (1328.), their particles freely conducting one to another. Yet when these enter into combination they form substances having no direct relation apparently, in this respect, to their elements; for water, sulphuric acid, and such compounds formed of insulating elements, conduct by comparison freely; whilst oxide of lead, flint glass, borate of lead, and other metallic compounds containing very high proportions of conducting matter, insulate excellently well. Taking oxide of lead therefore as the illustration, I conceive that it is not the particles of oxygen and lead which polarize separately under the act of induction, but the molecules of oxide of lead which exhibit this effect, all the elements of one particle of the resulting body, being held together as parts of one conducting individual by the bonds of chemical affinity, which is but another term for electrical force (918.).

1702. In bodies which are electrolytes we have still further reason for believing in such a state of things. Thus when water, chloride of tin, iodide of lead, &c. in the solid state are between the electrodes of the voltaic battery, their particles polarize as those of any other insulating dielectric do (1164.); but when the liquid state is conferred on these substances the polarized particles divide, the two halves, each in a highly charged state, travelling onwards until they meet other particles in an opposite and equally charged state, with which they combine, to the neutralization of their chemical, i. e. their electrical forces, and the reproduction of compound particles, which can again polarize as wholes, and again divide to repeat the same series of actions (1347.).

1703. But though electrolytic particles polarize as wholes, it would appear very evident that in them it is not a matter of entire indifference *how* the particle polarizes (1689.), since, when free to move (380, &c.) the polarities are ultimately distributed in reference to the elements; and sums of force equivalent to the polarities, and very definite in kind and amount, separate, as it were, from each other, and travel onwards with the elementary particles. And though I do not pretend to know what an atom is, or how it is associated or endowed with electrical force, or how this force is arranged in the cases of combination and decomposition, yet the strong belief I have in the electrical polarity of particles when under inductive action, and the bearing of such an opinion on the general effects of induction, whether ordinary or electrolytic, will be my excuse, I trust, for a few hypothetical considerations.

1704. In electrolyzation it appears that the polarized particles would (because of the gradual change which has been induced upon the chemical, i. e. the electrical forces of their elements (918.)) rather divide than discharge to each other without division (1348.); for if their division, i. e. their decomposition and recombination, be

prevented by giving them the solid state, then they will insulate electricity perhaps a hundredfold more intense than that necessary for their electrolyzation (419, &c.). Hence the tension necessary for direct conduction in such bodies appears to be much higher than that for decomposition (419. 1164. 1344.).

1705. The remarkable stoppage of electrolytic conduction by solidification (380. 1358.), is quite consistent with these views of the dependence of that process on the polarity which is common to all insulating matter when under induction, though attended by such peculiar electro-chemical results in the case of electrolytes. Thus it may be expected that the first effect of induction is so to polarize and arrange the particles of water that the positive or hydrogen pole of each shall be from the positive electrode and towards the negative electrode, whilst the negative or oxygen pole of each shall be in the contrary direction; and thus when the oxygen and hydrogen of a particle of water have separated, passing to and combining with other hydrogen and oxygen particles, unless these new particles of water could turn round they could not take up that position necessary for their successful electrolytic polarization. Now solidification, by fixing the water particles and preventing them from assuming that essential preliminary position, prevents also their electrolysis; and so the transfer of forces in that manner being prevented (1347. 1703.), the substance acts as an ordinary insulating dielectric (for it is evident by former experiments (419. 1704.) that the insulating tension is higher than the electrolytic tension), induction through it rises to a higher degree, and the polar condition of the molecules as wholes, though greatly exalted, is still securely maintained.

1706. When decomposition happens in a fluid electrolyte, I do not suppose that all the molecules in the same sectional plane (1634.) part with and transfer their electrified particles or elements at once. Probably the *discharge force* for that plane is summed up on one or a few particles, which decomposing, travelling and recombining, restore the balance of forces, much as in the case of spark disruptive discharge (1406.); for as those molecules resulting from particles which have just transferred power must by their position (1705.) be less favourably circumstanced than others, so there must be some which are most favourably disposed, and these, by giving way first, will for the time lower the tension and produce discharge.

1707. In former investigations of the action of electricity (821, &c.) it was shown, for many satisfactory cases, that the quantity of electric power transferred onwards was in proportion to and was definite for a given quantity of matter moving as anion or cathion onwards in the electrolytic line of action; and there was strong reason to believe that each of the particles of matter then dealt with, had associated with it a definite amount of electrical force, constituting its force of chemical affinity, the chemical equivalents and the electro-chemical equivalents being the same (836.). It was also found with few, and I may now perhaps say with no exceptions (1341.), that only those compounds containing elements in single proportions could exhibit the characters and phenomena of electrolytes (697.); oxides, chlorides, and other bodies

containing more than one proportion of the electro-negative element refusing to decompose under the influence of the electric current.

1708. Probable reasons for these conditions and limitations arise out of the molecular theory of induction. Thus when a liquid dielectric, as chloride of tin, consists of molecules, each composed of a single particle of each of the elements, then as these can convey equivalent opposite forces by their separation in opposite directions, both decomposition and transfer can result. But when the molecules, as in the bichloride of tin, consist of one particle or atom of one element, and two of the other, then the simplicity with which the particles may be supposed to be arranged and to act, is destroyed. And, though it may be conceived that when the molecules of bichloride of tin are polarized as wholes by the induction across them, the positive polar force might accumulate on the one particle of tin whilst the negative polar force accumulated on the two particles of chlorine associated with it, and that these might respectively travel right and left to unite with other two of chlorine and one of tin, in analogy with what happens in cases of compounds consisting of single proportions, yet this is not altogether so evident or probable. For when a particle of tin combines with two of chlorine, it is difficult to conceive that there should not be some relation of the three in the resulting molecule analogous to fixed position, the one particle of metal being perhaps symmetrically placed in relation to the two of chlorine: and, it is not difficult to conceive of such particles that they could not assume that position dependent both on their polarity and the relation of their elements, which appears to be the first step in the process of electrolyzation (1345. 1705.).

§. 21. *Relation of the electric and magnetic forces.*

1709. I have already ventured a few speculations respecting the probable relation of magnetism, as the transverse force of the current, to the divergent or transverse force of the lines of inductive action belonging to static electricity (1658, &c.).

1710. In the further consideration of this subject it appeared to me to be of the utmost importance to ascertain, if possible, whether this lateral action which we call magnetism, or sometimes the induction of electrical currents (26. 1048, &c.), is extended to a distance *by the action of the intermediate particles* in analogy with the induction of static electricity, or the various effects, such as conduction, discharge, &c., which are dependent on that induction; or, whether its influence at a distance is altogether independent of such intermediate particles (1662.).

1711. I arranged two magneto-electric helices with iron cores end to end, but with an interval of an inch and three quarters between them, in which interval was placed the end or pole of a bar magnet. It is evident that on moving the magnetic pole from one core towards the other, a current would tend to form in both helices, in the one because of the lowering, and in the other because of the strengthening of the magnetism induced in the respective soft iron cores. The helices were connected together, and also with a galvanometer, so that these two currents should coincide in

direction, and tend by their joint force to deflect the needle of the instrument. The whole arrangement was so effective and delicate, that moving the magnetic pole about the eighth of an inch to and fro two or three times, in periods equal to those required for the vibrations of the galvanometer needle, was sufficient to cause considerable vibration in the latter; thus showing readily the consequence of strengthening the influence of the magnet on the one core and helix, and diminishing it on the other.

1712. Then without disturbing the distances of the magnet and cores, plates of substances were interposed. Thus calling the two cores A and B, a plate of shell-lac was introduced between the magnetic pole and A for the time occupied by the needle in swinging one way; then it was withdrawn for the time occupied in the return swing; introduced again for another equal portion of time; withdrawn for another portion, and so on eight or nine times; but not the least effect was observed on the needle. In other cases the plate was alternated, i. e. it was introduced between the magnet and A for one period of time, withdrawn and introduced between the magnet and B for the second period, withdrawn and restored to its first place for the third period, and so on, but with no effect on the needle.

1713. In these experiments *shell-lac* in plates 0·9 of an inch in thickness, *sulphur* in a plate 0·9 of an inch in thickness, and *copper* in a plate 0·7 of an inch in thickness were used without any effect. And I conclude that bodies, contrasted by the extremes of conducting and insulating power, and opposed to each other as strongly as metals, air, and sulphur, show no difference with respect to magnetic forces when placed in their lines of action, at least under the circumstances described.

1714. With a plate of iron, or even a small piece of that metal, as the head of a nail, a very different effect was produced, for then the galvanometer immediately showed its sensibility, and the perfection of the general arrangement.

1715. I arranged matters so that a plate of *copper* 0·2 of an inch in thickness, and ten inches in diameter, should have the part near the edge interposed between the magnet and the core, in which situation it was first rotated rapidly, and then held quiescent alternately, for periods according with that required for the swinging of the needle; but not the least effect upon the galvanometer was produced.

1716. A plate of shell-lac 0·6 of an inch in thickness was applied in the same manner, but whether rotating or not it produced no effect.

1717. Occasionally the plane of rotation was directly across the magnetic curve: at other times it was made as oblique as possible; the direction of the rotation being also changed in different experiments, but not the least effect was produced.

1718. I now removed the helices with their soft iron cores, and replaced them by two *flat helices* wound upon card board, each containing forty-two feet of silked copper wire, and having no associated iron. Otherwise the arrangement was as before, and exceedingly sensible; for a very slight motion of the magnet between the helices produced an abundant vibration of the galvanometer needle.

1719. The introduction of plates of shell-lac, sulphur, or copper into the intervals

between the magnet and these helices (1713.), produced not the least effect, whether the former were quiescent or in rapid revolution (1715.). So here no evidence of the influence of the intermediate particles could be obtained (1710.).

1720. The magnet was then removed and replaced by a flat helix, corresponding to the two former, the three being parallel to each other. The middle helix was so arranged that a voltaic current could be sent through it at pleasure. The former galvanometer was removed, and one with a double coil employed, one of the lateral helices being connected with one coil, and the other helix with the other coil, in such manner that when a voltaic current was sent through the middle helix its inductive action (26.) on the lateral helices should cause currents in them, having contrary directions in the coils of the galvanometer. By a little adjustment of the distances these induced currents were rendered exactly equal, and the galvanometer needle remained stationary notwithstanding their frequent production in the instrument. I will call the middle coil C, and the external coils A and B.

1721. A plate of copper 0·7 of an inch thick and six inches square, was placed between coils C and B, their respective distances remaining unchanged; and then a voltaic current from twenty pairs of 4-inch plates was sent through the coil C, and intermitted, in periods fitted to produce an effect on the galvanometer (1712.), if any difference had been produced in the effect of C on A and B. But notwithstanding the presence of air in one interval and copper in the other, the inductive effect was exactly alike on the two coils, and as if air had occupied both intervals. So that notwithstanding the facility with which any induced currents might form in the thick copper plate, the coil outside of it was just as much affected by the central helix C as if no such conductor as the copper had been there.

1722. Then, for the copper plate was substituted one of sulphur 0·9 of an inch thick; still the results were exactly the same, i. e. there was no action at the galvanometer.

1723. Thus it appears that when a voltaic current in one wire is exerting its inductive action to produce a contrary or a similar current in a neighbouring wire, according as the primary current is commencing or ceasing, it makes not the least difference whether the intervening space is occupied by such insulating bodies as air, sulphur and shell-lac, or such conducting bodies as copper, and the other non-magnetic metals.

1724. A correspondent effect was obtained with the like forces when resident in a magnet thus. A single flat helix (1718.) was connected with a galvanometer, and a magnetic pole placed near to it; then by moving the magnet to and from the helix, or the helix to and from the magnet, currents were produced indicated by the galvanometer.

1725. The thick copper plate (1721.) was afterwards interposed between the magnetic pole and the helix; nevertheless on moving these to and fro, effects, exactly the same in direction and amount, were obtained as if the copper had not been there. So also on introducing a plate of sulphur into the interval, not the least influence on the currents produced by motion of the magnet or coils could be obtained.

1726. These results, with many others which I have not thought it needful to de-

scribe, would lead to the conclusion that (judging by the amount of effect produced at a distance by forces transverse to the electric current, i. e. magnetic forces,) the intervening matter, and therefore the intervening particles, have nothing to do with the phenomena; or in other words, that though the inductive force of static electricity is transmitted to a distance by the action of the intermediate particles (1164. 1666.), the transverse inductive force of currents, which can also act at a distance, is not transmitted by the intermediate particles in a similar way.

1727. It is however very evident that such a conclusion cannot be considered as proved. Thus when the metal copper is between the pole and the helix (1715. 1719. 1725.) or between the two helices (1721.) we know that its particles are affected, and can by proper arrangements make their peculiar state for the time very evident by the production of either electrical or magnetical effects. It seems impossible to consider this effect on the particles of the intervening matter as independent of that produced by the inductric coil or magnet C, on the inducteous coil or core A (1715. 1721.); for since the inducteous body is equally affected by the inductric body whether these intervening and affected particles of copper are present or not (1723. 1725.), such a supposition would imply that the particles so affected had no reaction back on the original inductric forces. The more reasonable conclusion, as it appears to me, is, to consider these affected particles as efficient in continuing the action onwards from the inductric to the inducteous body, and by this very communication producing the effect of no loss of induced power at the latter.

1728. But then it may be asked what is the relation of the particles of insulating bodies, such as air, sulphur, or lac, when *they* intervene in the line of magnetic action? The answer to this is at present merely conjectural. I have long thought there must be a particular condition of such bodies corresponding to the state which causes currents in metals and other conductors (26. 53. 191. 201. 213.); and considering that the bodies are insulators one would expect that state to be one of tension. I have by rotating non-conducting bodies near magnetic poles and poles near them, and also by causing powerful electric currents to be suddenly formed and to cease around and about insulators in various directions, endeavoured to make some such state sensible, but have not succeeded. Nevertheless, as any such state must be of exceedingly low intensity, because of the feeble intensity of the currents which are used to induce it, it may well be that the state may exist, and may be discoverable by some more expert experimentalist, though I have not been able to make it sensible.

1729. It appears to me possible, therefore, and even probable, that magnetic action may be communicated to a distance by the action of the intervening particles, in a manner having a relation to the way in which the inductive forces of static electricity are transferred to a distance (1677.); the intervening particles assuming for the time more or less of a peculiar condition, which (though with a very imperfect idea) I have several times expressed by the term *electro-tonic state* (60. 242. 1114. 1661.). I hope it will not be understood that I hold the settled opinion that such is the case. I would

rather in fact have proved the contrary, namely, that magnetic forces are quite independent of the matter intervening between the inductric and the inducteous bodies; but I cannot get over the difficulty presented by such substances as copper, silver, lead, gold, carbon, and even aqueous solutions (201. 213.), which though they are known to assume a peculiar state whilst intervening between the bodies acting and acted upon (1727.), no more interfere with the final result than those which have as yet had no peculiarity of condition discovered in them.

1730. A remark important to the whole of this investigation ought to be made here. Although I think the galvanometer used as I have described it (1711. 1720.) is quite sufficient to prove that the final amount of action on each of the two coils or the two cores A and B (1713. 1719.) is equal, yet there is an effect which *may* be consequent on the difference of action of two interposed bodies which it would not show. As time enters as an element into these actions* (125.), it is very possible that the induced actions on the helices or cores A, B, though they rise to the same degree when air and copper, or air and lac are contrasted as intervening substances, do not do so in the same time; and yet, because of the length of time occupied by a vibration of the needle, this difference may not be visible, both effects rising to their maximum in periods so short as to make no sensible portion of that required for a vibration of the needle, and so exert no visible influence upon it.

1731. If the lateral or transverse force of electrical currents, or what appears to be the same thing, magnetic power, could be proved to be influential at a distance independently of the intervening contiguous particles, then, as it appears to me, a real distinction, of a high and important kind, would be established between the natures of these two forces (1654. 1664.). I do not mean that the powers are independent of each other and might be rendered separately active, on the contrary they are probably essentially associated (1654.), but it by no means follows that they are of the same nature. In common statical induction, in conduction, and in electrolyzation, the forces at the opposite extremities of the particles which coincide with the lines of action, and have commonly been distinguished by the term electric, are polar, and in the cases of contiguous particles act only to insensible distances; whilst those which are transverse to the direction of these lines, and are called magnetic, are circumferential, act at a distance, and if not through the intermediation of the intervening particles, have their relations to ordinary matter entirely unlike those of the electrical forces with which they are associated.

1732. To decide this question of the identity or distinction of the two kinds of power, and establish their true relation, would be exceedingly important. The question seems fully within the reach of experiment, and offers a high reward to him who will attempt its settlement.

* See Annales de Chimie, 1833, tom. li. pp. 422, 428.

1733. I have already expressed a hope of finding an effect or condition which shall be to statical electricity what magnetic force is to current electricity (1658.) If I could have proved to my own satisfaction that magnetic forces extended their influence to a distance by the conjoined action of the intervening particles in a manner analogous to that of electrical forces, then I should have thought that the lateral tension of the lines of inductive action (1659.), or that state so often hinted at as the electro-tonic state (1661. 1662.), was this related condition of statical electricity.

1734. It may be said that the state of *no lateral action* is to static or inductive force the equivalent of *magnetism* to current force; but that can only be upon the view that electric and magnetic action are in their nature essentially different (1664.). If they are the same power, the whole difference in the results being the consequence of the difference of *direction*, then the normal or *undeveloped* state of electric force will correspond with the state of *no lateral action* of the magnetic state of the force; the electric current will correspond with the lateral effects commonly called magnetism: but the state of static induction which is between the normal condition and the current will still require a corresponding lateral condition in the magnetic series, presenting its own peculiar phenomena; for it can hardly be supposed that the normal electric and the inductive or polarized electric condition can both have the same lateral relation. If magnetism be a separate and a higher relation of the powers developed, then perhaps the argument which presses for this third condition of that force would not be so strong.

1735. I cannot conclude these general remarks upon the relation of the electric and magnetic forces without expressing my surprise at the results obtained with the copper plate (1721. 1725.). The experiments with the flat helices represent one of the simplest cases of the induction of electrical currents (1720.); the effect, as is well known, consisting in the production of a momentary current in a wire at the instant when a current in the contrary direction begins to pass through a neighbouring parallel wire, and the production of an equally brief current in the reverse direction when the determining current is stopped (26.). Such being the case, it seems very extraordinary that this induced current which takes place in the helix A when there is only air between A and C (1720.) should be equally strong when that air is replaced by an enormous mass of that excellently conducting metal copper (1721.). It might have been supposed that this mass would have allowed of the formation and discharge of almost any quantity of currents in it, which the helix C was competent to induce, and so in some degree have diminished if not altogether prevented the effect in A: instead of which, though we can hardly doubt that an infinity of currents are formed at the moment in the copper plate, still not the smallest diminution or alteration of the effect in A appears. Almost the only way of reconciling this effect with generally received notions is, as it appears to me, to admit that magnetic action is communicated by the action of the intervening particles (1729. 1733.).

1736. This condition of things, which is very remarkable, accords perfectly with

the effects observed in solid helices where wires are coiled over wires to the amount of five or six or more layers in succession, no diminution of effect on the outer ones being occasioned by those within.

§. 22. *Note on electrical excitation.*

1737. That the different modes in which electrical excitement takes place will some day or other be reduced under one common law can hardly be doubted, though for the present we are bound to admit distinctions. It will be a great point gained when these distinctions are, not removed but, understood.

1738. The strict relation of the electrical and chemical powers renders the chemical mode of excitement the most instructive of all, and the case of two isolated combining particles is probably the simplest that we possess. Here however the action is local, and we still want such a test of electricity as shall apply to it, to cases of current electricity, and also to those of static induction. Whenever by virtue of the previously combined condition of some of the acting particles (923.) we are enabled, as in the voltaic pile, to expand or convert the local action into a current, then chemical action can be traced through its variations to the production of *all* the phenomena of tension and the static state, these being in every respect the same as if the electric forces producing them had been developed by friction.

1739. It was Berzelius, I believe, who first spoke of the aptness of certain particles to assume opposite states when in presence of each other (959.). Hypothetically we may suppose these states to increase in intensity by increased approximation, or by heat, &c. until at a certain point combination occurs, accompanied by such an arrangement of the forces of the two particles between themselves as is equivalent to a discharge, producing at the same time a particle which is throughout a conductor (1700.).

1740. This aptness to assume an excited electrical state (which is probably polar in those forming non-conducting matter) appears to be a primary fact, and to partake of the nature of induction (1162.), for the particles do not seem capable of retaining their particular state independently of each other (1177.) or of matter in the opposite state. What appears to be definite about the particles of matter is their assumption of a *particular* state, as the positive or negative, in relation to each other, and not of either one or other indifferently; and also the acquirement of force up to a certain amount.

1741. It is easily conceivable that the same force which causes local action between two free particles shall produce current force if one of the particles is previously in combination, forming part of an electrolyte (923. 1738.). Thus a particle of zinc, and one of oxygen, when in presence of each other, exert their inductive forces (1740.), and these at last rise up to the point of combination. If the oxygen be previously in union with hydrogen, it is held so combined by an analogous exertion and arrangement of the forces; and as the forces of the oxygen and hydrogen are for the time of combination mutually engaged and related, so when the superior relation of the forces be-

tween the oxygen and zinc come into play, the induction of the former or oxygen towards the metal cannot be brought on and increased without a corresponding deficiency in its induction towards the hydrogen with which it is in combination (for the amount of force in a particle is considered as definite), and the latter therefore has its force turned towards the oxygen of the next particle of water; thus the effect may be considered as extended to sensible distances, and thrown into the condition of static induction, which being discharged and then removed by the action of other particles produces currents.

1742. In the common voltaic battery, the current is occasioned by the tendency of the zinc to take the oxygen of the water from the hydrogen, the effective action being at the place where the oxygen leaves the previously existing electrolyte. But SCHŒNBEIN has arranged a battery in which the effective action is at the other extremity of this essential part of the arrangement, namely, where oxygen goes to the electrolyte. The first may be considered as a case where the current is put into motion by the abstraction of oxygen from hydrogen, the latter by that of hydrogen from oxygen. The direction of the electric current is in both cases the same, when referred to the direction in which the elementary particles of the electrolyte are moving (923. 962.), and both are equally in accordance with the hypothetical view of the inductive action of the particles just described (1740.).

1743. In such a view of voltaic excitement, the action of the particles may be divided into two parts, that which occurs whilst the force in a particle of oxygen is rising towards a particle of zinc acting on it, and falling towards the particle of hydrogen with which it is associated (this being the progressive period of the inductive action), and that which occurs when the change of association takes place, and the particle of oxygen leaves the hydrogen and combines with the zinc. The former appears to be that which produces the current, or if there be no current, produces the state of tension at the termination of the battery; whilst the latter, by terminating for the time the influence of the particles which have been active, allows of others coming into play, and so the effect of current is continued.

1744. It seems highly probable, that excitement by friction may very frequently be of the same character. WOLLASTON endeavoured to refer such excitement to chemical action*; but if by chemical action ultimate union of the acting particles is intended, then there are plenty of cases which are opposed to such a view. DAVY mentions some such, and for my own part I feel no difficulty in admitting other means of electrical excitement than chemical action, especially if by chemical action is meant a final combination of the particles.

1745. DAVY refers experimentally to the opposite states which two particles having opposite chemical relations can assume when they are brought into the close vicinity of each other, but *not* allowed to combine†. This, I think, is the first part of the action already described (1743.); but in my opinion it cannot give rise to a con-

* Philosophical Transactions, 1801, p. 427. † Ibid. 1807. p. 34.

tinuous current unless combination take place, so as to allow other particles to act successively in the same manner, and not even then unless one set of the particles be present as an element of an electrolyte (923. 963.); i. e. mere quiescent contact alone without chemical action does not in such cases produce a *current*.

1746. Still it seems very possible that such a relation may produce a high charge, and thus give rise to excitement by friction. When two bodies are rubbed together to produce electricity in the usual way, one at least must be an insulator. During the act of rubbing, the particles of opposite kinds must be brought more or less closely together, the few which are most favourably circumstanced being in such close contact as to be short only of that which is consequent upon chemical combination. At such moments they may acquire by their mutual induction (1740.) and partial discharge to each other very exalted opposite states, and when, the moment after, they are by the progress of the rub removed from each other's vicinity, they will retain this state if both bodies be insulators, and exhibit them upon their complete separation.

1747. All the circumstances attending friction seem to me to favour such a view. The irregularities of form and pressure will cause that the particles of the two rubbing surfaces will be at very variable distances, only a few at once being in that very close relation which is probably necessary for the development of the forces; further, those which are nearest at one time will be further removed at another, and others will become the nearest, and so by continuing the friction many will in succession be excited. Finally, the lateral direction of the separation in rubbing seems to me the best fitted to bring many pairs of particles, first of all into that close vicinity necessary for their assuming the opposite states by relation to each other, and then to remove them from each other's influence whilst they retain that state.

1748. It would be easy, on the same view, to explain hypothetically, how, if one of the rubbing bodies be a conductor, as the amalgam of an electrical machine, the state of the other when it comes from under the friction is (as a mass) exalted; but it would be folly to go far into such speculation before that already advanced has been backed or corrected by fit experimental evidence. I do not wish it to be supposed that I think all excitement by friction is of this kind; on the contrary, certain experiments lead me to believe that in many cases, and perhaps in all, effects of a thermo-electric nature conduce to the ultimate end; and there are very probably other causes of electric disturbance influential at the same time, which we have not as yet distinguished.

Royal Institution,
June, 1838.

XIV. *An Account of some Experiments on the Blood in connexion with the Theory of Respiration.* By John Davy, *M.D. F.R.S., Assistant Inspector of Army Hospitals.*

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CONNECTED with the theory of respiration and of animal heat there are many questions of interest respecting the blood, about which physiologists differ in opinion, and which consequently are fit subjects for further inquiry.

Some of the more important and fundamental of these questions I have endeavoured to investigate experimentally, and in the present communication I propose to submit to the Royal Society the results which I have obtained, with the hope that they may be considered not unworthy of a place in the Philosophical Transactions.

I. *Is blood capable of absorbing oxygen independent of putrefaction?*

To endeavour to satisfy myself on this point, on which in a former inquiry I had arrived at a negative conclusion in opposition to the commonly received opinion, I have employed two methods of experimenting: one, of agitating blood, recently drawn and rapidly cooled in common air and in oxygen, in a tube of the capacity of two cubic inches, divided into a hundred parts; the other, of agitating it in larger quantities with the same airs, in the very convenient apparatus employed by Dr. Christison when engaged in a similar inquiry, consisting of a double tubulated bottle of the capacity of thirty-two cubic inches, provided with stop-cocks adapted by grinding, to one of which a moveable bent glass tube was fitted to connect it with a pneumatic trough, and to the other a perpendicular tube surmounted by a funnel.

The blood subjected to experiment in every instance was prepared by the displacement of its fibrin. This was done by agitating it with small pieces of sheet-lead in a bottle filled to overflowing and closed with a cork, enveloped in moist bladder and covered over with the same, tied round the neck of the bottle, so as to exclude atmospheric air, whilst in the act of coagulating and of cooling, and to allow, when cooled, of the withdrawal of the cork.

Prepared thus, and rapidly cooled, I have tried different specimens of blood, venous and arterial, of Man, of the Sheep, Ox, Dog and Cat; and the results, when the blood has been taken from a healthy animal, have been decisive and consistent. In every instance, whether atmospheric air or oxygen was used, after agitation, there was a marked diminution of the volume of air.

In the examples which it may be advisable to bring forward, I shall confine myself to a few of the experiments I have made on the blood of the Sheep.

Using the graduated tube over mercury, sixty-two measures of arterial blood from

the carotid artery, agitated with thirty-three measures of common air, produced a diminution of two measures; and sixty-three of the same arterial blood, agitated with thirteen of pure oxygen, a diminution of three; whilst sixty-three of venous blood from the jugular vein of the same animal, agitated with thirty-three of common air, produced a diminution of six; and seventy of this blood, with thirteen of oxygen, a diminution of eight*.

In experiments on a larger scale, using the double-mouthed bottle, in which about ten cubic inches of blood were agitated with about twenty-two cubic inches of air, the results were in accordance with the preceding. Thus when the arterial blood of the Sheep was agitated with common air and with oxygen, on turning the stop-cock of the bent tube there was an absorption in one instance of about ·3 cubic inch, and in the other of about ·4 cubic inch; and, with venous blood, in the instance of common air, of about 1· cubic inch, and in that of oxygen, of about 1·25 cubic inch.

These experiments were all made on the blood of the same animal. In experiments on the blood of different individuals of the same species of animal, and on the blood of animals of different species, the results have varied in regard to the degree of absorption, and remarkably so in the instance of the blood of man.

In every instance the absorption or disappearance of a portion of the air has been attended with some change in the colour of the blood; the venous has invariably acquired the florid vermillion hue of arterial blood, and the arterial has had its florid hue heightened.

The air that has been absorbed or which disappears, when atmospheric air has been used, in accordance with the commonly received opinion and the results of Dr. Christison's experiments†, has been found to be oxygen.

Relative to the residual air, when pure oxygen has been used, whether on the smaller or larger scale of experiment, no carbonic acid gas has been detected in it in the most carefully conducted trials. When common air has been employed, then a trace of this acid gas has been found in the residual air after agitation with the blood, but not exceeding 1 per cent. at a temperature between 40° and 50°. I shall notice in detail an experiment with each, as the results are of consequence.

Ten cubic inches of venous blood (its fibrin displaced) from the jugular vein of a Sheep, rapidly cooled, were agitated in twenty-two cubic inches of oxygen gas, which had been well washed with lime-water. After an absorption equal to about one cubic inch, some of the gas was expelled and passed into lime-water, the transparency of which it did not in the slightest degree impair.

The same quantity of similar blood was similarly treated with common air, which

* Notwithstanding the frothing attending the agitation of blood in air, the absorption or diminution of volume was ascertained with tolerable accuracy by observing the rise of the mercury in the tube. The experiments were made as nearly as possible under ordinary atmospheric pressure, which was easily effected, as the mercurial pneumatic trough used exceeded in depth the length of the tube.

† Edinburgh Medical and Surgical Journal, vol. xxxv. p. 94.

also had been washed with lime-water. After an absorption equal to about one cubic inch, some of the air was passed into lime-water: it occasioned a just perceptible cloudiness; and eighty measures of it agitated with lime-water were reduced hardly to 79·5.

Taking it for granted that this very minute quantity of carbonic acid derived from the blood existed in the state of gas, and not contained in the aqueous vapour, as is possible, it is matter for consideration from whence it was derived,—whether it was formed at the moment by the action of the air on the blood, or, previously existing in the blood, was now merely expelled. Further on I propose to return to this important question.

When I reflect on the results stated above relative to the absorption or disappearance of oxygen, and compare them with those alluded to formerly obtained, I am not a little surprised at their discrepancy; and I can only account for it by supposing that it may have been owing in part to the difference of season when the two sets of experiments were conducted. The first were made in Malta in 1829, in the hot months of July and August, when the thermometer in the open air was generally above 80° and occasionally above 90°. The last have been made in England, and principally in January of the present year, when the temperature of atmosphere was occasionally low, the greater part of the time below the freezing point, and often as low as 20°. From what I have witnessed, I am induced to infer, that the higher the atmospheric temperature is, and the less necessity there is for the production of animal heat, the less difference there is between venous and arterial blood, and the less power the former has of combining with oxygen and of forming or evolving carbonic acid. In Malta I carefully compared the blood of the jugular vein and carotid artery of a Sheep during the season mentioned; when coagulated and still hot, there was no perceptible difference in their colour; in each it was less florid than the arterial blood of the same animal in England in winter, and less dark than the venous: its hue was as it were a mixture of the two. And in this observation I could not be mistaken; the circumstances under which it was made precluded mistake; vessels of the same size were used; similar quantities of blood were introduced, and they were seen in the same light side by side. And in confirmation of this view I may remark, that during the last winter, when the cold was unusually severe, I found the temperature of deeply seated parts, and especially of the heart and its left ventricle, in the instance of Sheep, unusually high: the mean of nine observations on the temperature of the left ventricle in different animals was 107·5; the lowest was 105·5; the highest 109·; whilst the temperature of the rectum (the mean of the same number of observations) was 104·4.

II. *Does the blood, especially venous blood, contain carbonic acid capable of being expelled by agitation with another gas, as hydrogen or oxygen?*

In a paper published in the Philosophical Transactions for 1832, Dr. Stevens has answered this question in the affirmative: he maintains that carbonic acid gas exists

in venous blood; that it may be expelled by oxygen or by hydrogen, although not by the air-pump; and he supposes that the difference of effect is owing to a peculiar attraction for carbonic acid exercised by these gases.

To endeavour to resolve my doubts on this important point, I have had recourse to the apparatus already mentioned, viz. the graduated tube with the mercurial pneumatic trough, and the double-mouthed bottle furnished with stop-cocks, &c. as being well adapted for simple and decisive experiments.

By means of the graduated tube I have agitated venous blood in hydrogen over mercury, as about a cubic inch of each, and other proportions, and have left the blood exposed to the influence of the gas for several hours; and I have made similar trials with it, using larger quantities in the double-mouthed bottle, as sixteen cubic inches of each, and also other proportions; the results have been either of the same negative character, or, if different, indicating only the disengagement of carbonic acid gas in an extremely minute quantity. In all the experiments with the graduated tube in which fresh blood was used, whether of Man or of the Sheep, the fibrin displaced out of the contact of the air, on agitation with hydrogen, there was no sensible increase of the volume of the gas, and no diminution of it when it was transferred to, and shaken with, lime-water. And in the best experiments, on a larger scale, with the double-mouthed bottle, when most attention was paid to all the circumstances likely to insure accuracy, as in the first instance the exclusion of air from the blood, and in the second the having it of the temperature of the bottle and of the room, the results have been similar, and of a negative character. I shall describe a small number of experiments, those, the results of which appeared least ambiguous.

Seven cubic inches of Sheep's blood from the jugular vein, its fibrin broken up by agitation with lead whilst coagulating, out of contact with air, and cooled under water, were agitated with hydrogen (twenty-five cubic inches), previously well washed with lime-water, and which, tested by lime-water, after this precaution were found perfectly free from carbonic acid. On turning the stop-cock of the bent tube connected with distilled water, no change of volume was indicated, and the blood was agitated again with the same result. By means of the perpendicular tube distilled water was admitted, and some of the gas expelled; first a cubic inch into a graduated tube filled with lime-water, and next about four cubic inches into a vial filled with distilled water, and in which afterwards a little lime-water was added to the gas; in neither could any traces of carbonic acid be detected; the lime-water remained transparent.

Ten cubic inches of venous blood, taken by a large orifice from the arm of a young Man threatened with hæmoptysis, the fibrin broken up in the same manner as the last, and rapidly cooled under water in a running stream to the temperature of the room, 51°, were similarly treated with hydrogen, and with precisely the same result; after having been twice well shaken, on turning the stop-cock, there was no change of volume. The blood was kept in contact with the hydrogen over night, the stop-

cocks closed. The night, that of the 23rd of January, was severely cold; at 11 o'clock the following morning the temperature of the room was only 45°; now on turning the stop-cock of the bent tube the water rose in it to the extent of about one-eighth of a cubic inch.

Twenty-four cubic inches of the mixed arterial and venous blood of the Sheep, collected and prepared with similar precautions, were divided into two portions of about twelve cubic inches; one was agitated in the double-mouthed bottle with hydrogen after the introduction of a little milk of lime, the other without this addition. The result in each instance was the same; on opening the stop-cock, after the agitation, the water rose a very little in the bent tube, about one-twentieth of a cubic inch.

The results of some of the trials already described on the action of oxygen on venous blood, both pure and mixed with azote, in the form of common air, are very consistent with those just detailed on hydrogen. Previously to stating some other results in quest of fresh evidence on the same subject, it may be advisable to notice particularly the power which blood possesses of absorbing carbonic acid.

From experiments which I made on blood and serum in 1824 and 1828*, I inferred that each is capable of absorbing about an equal volume of this acid gas. I now find that when pure carbonic acid gas is brought in contact with blood or serum over mercury, and moderately agitated under ordinary atmospheric pressure, that the absorption of gas exceeds the volume of the fluid, both in the instance of blood and serum. The results of some experiments are exhibited in the following Table. The majority of them were obtained between a temperature of 40° and 45°; the three last at about 51°. When the venous and arterial blood, and the serum tried, were from the same animal, the numbers expressing the results are entered in the same line.

Volume of carbonic acid gas absorbed by 100 *parts of blood and serum.*

No.	Animal.	Venous blood.	Venous serum.	Arterial blood.	Arterial serum.
1	Sheep.....	160	.....	167	
2	Sheep.....	155			
3	Sheep.....	.....	.....	133	
4	Sheep.....	142			
5	Sheep.....	.....	.....	150	
6	Sheep.....	166			
7	Sheep.....	.....	.....	.....	120
8	Ox.....	194			
9	Ox.....	181			
10	Man	118			
11	Heifer.....	120	.....	.....	117
12	Sheep.....	148	125	141	125
13	Sheep.....	.....	118	.....	118
14	Man	153			
15	Sheep.....	160			
16	Sheep.....	.....	.....	159	

* Philosophical Transactions for 1824. Edinburgh Medical and Surgical Journal, vol. xxx.

The effect of the absorption of the gas to perfect saturation was on the arterial and venous blood the same; it rendered both very dark; the serum it rendered more liquid, which was well marked by diminished tendency to froth on agitation.

I shall now proceed to notice the trials which I have instituted of agitating blood and serum, to which a known quantity of carbonic acid had been added, with one or more of the gases considered by Dr. Stevens as exerting an attraction on carbonic acid, and by that means expelling it.

From the experiments which I have made on serum, it appears in its healthy state incapable of absorbing oxygen, or of immediately furnishing carbon to form carbonic acid; and in no instance in which I have agitated it with common air or with hydrogen, when obtained from the blood of a healthy animal, has there been any indication of the disengagement of gas; it therefore is peculiarly well fitted for the trial in question.

To nine cubic inches of serum from the mixed blood of the Sheep one cubic inch more of serum was added, containing a cubic inch of carbonic acid, with which it had been impregnated over mercury. The mixture of the two was introduced into the double-mouthed bottle without delay, and well agitated with twenty-two cubic inches of common air. On turning the stop-cock there was no change of volume. The serum was transferred, and there was added to it, with as little motion as possible, another cubic inch of serum, containing the same quantity of carbonic acid. Now poured back into the bottle and agitated, on opening the stop-cock a little air was disengaged; it was collected and found equal to $\frac{1}{100}$ths cubic inch. The serum was left exposed to the action of the air in the bottle over night, the stop-cocks closed; the following morning on opening the stop-cock of the bent tube no air was expelled; on the contrary, there was a just perceptible rise of water in it. The experiment was carried further: the serum was transferred to a vial and closed, and the double-mouthed bottle was filled with hydrogen. The serum was returned and well agitated with the hydrogen. On turning the stop-cock $\frac{1}{100}$ths of a cubic inch of air was expelled. It was agitated a second time without further expulsion of air, and left in contact with hydrogen for more than twelve hours without any further effect. Thus it appears that of the two cubic inches of carbonic acid gas introduced into the serum, only one-fifth of a cubic inch was expelled by successive agitation with atmospheric air and with hydrogen.

One cubic inch of venous blood of a Man which had absorbed 1·2 cubic inch of carbonic acid, was mixed with twelve cubic inches of similar blood, and agitated with hydrogen in the double-mouthed bottle. A very little air only was expelled, viz. $\frac{1}{100}$ths cubic inch.

To fifty-five measures (1·1 cubic inch) of venous blood of a Sheep, twenty measures of gas were added over mercury, composed of about equal parts of oxygen and carbonic acid. After agitation about seventeen measures were absorbed, and the blood had acquired a florid hue; ten measures more of oxygen were added; there was no

further absorption. The tube was transferred to lime-water and agitated; the residual air was not diminished; it amounted to thirteen or fourteen measures (the froth prevented precision in marking the quantity), which possessed the properties of oxygen, as tested by the taper.

To twenty-eight measures of venous blood of a Sheep, which had absorbed twenty-six of carbonic acid, forty-nine of oxygen were added in the graduated tube over mercury: after agitation the blood had acquired the florid arterial hue, and there appeared to be an expansion of one measure. Transferred to lime-water there was an absorption of one or two measures, and no more.

I shall notice one experiment more, in which blood nearly saturated with carbonic acid was exposed to oxygen, a membrane intervening. Forty-seven measures of venous blood, which had absorbed thirty-three of carbonic acid, were introduced into a glass tube half an inch in diameter, closed at one end with gold-beater's skin, and when filled with blood at the other end also, it was placed over mercury in a small receiver, and thirty-seven measures of oxygen were added to it, under a diminished pressure of about one inch. After twenty-four hours there was no change of volume; the blood in the tube had acquired throughout the arterial hue; the gas, thirty-eight measures, transferred to lime-water and agitated, diminished to thirty-two.

The tube was now placed under a receiver, and the air exhausted by the air-pump; a good deal of air was disengaged in the form of froth, and the gold-beater's skin was so distended that it appeared ready to burst; after three or four minutes air was re-admitted; a notable portion of gas was found free between the membrane and blood; thus showing that in oxygen gas carbonic acid gas is less freely exhaled through a membrane than in vacuo.

The results of this second set of experiments are in accordance with those of the first. The inference I am induced to draw from both is rather of a negative kind, and unfavourable to the conclusion of Dr. Stevens already referred to, at least in a strict and general sense. I think it right to express myself thus reservedly, reflecting on some of the experiments in which a very little carbonic acid gas appeared to be extricated on agitating blood with hydrogen; and believing that Dr. Stevens, and other able inquirers, could not have been misled on a point so little exposed to fallacy.

Relative to the effects which Dr. Stevens refers to the attraction of one gas for another, they appear to me, from what I have witnessed in carrying on the inquiry, to admit of explanation on Dr. Dalton's theory of mixed gases, and that in no instance is the effect of disengagement of air from a fluid, agitated with another kind of air, greater than were it agitated in a vacuum.

III. *What is the condition of the alkali in the blood in relation to carbonic acid?*

On this point much difference of opinion exists amongst inquirers; some believing that the alkali, or at least a portion of it, is uncombined, or combined merely with water, or with water and albumen; some that it is united to carbonic acid in the

state of carbonate; others that it is saturated with this gas, and in the state of bicarbonate.

The subject, it must be confessed, is one of great difficulty, and very perplexing; partly from the nature of the blood, liable to great variations during life, and to rapid change after death; and partly also from the nature of the alkaline carbonates, hardly less disposed to change than the blood itself, from variation of circumstances, and to pass from one degree of combination into another.

The bicarbonate of soda, I believe, like the bicarbonate of ammonia, can only exist in perfection in the solid state. In dissolving, I find, when exposed to the atmosphere, it gives off a part of its acid, and still more when it is agitated with common air, and more still with hydrogen, and in a greater degree the higher the temperature. This is not favourable to the idea that it exists in the blood, especially when it is considered that this fluid may be exposed to a temperature of 212° without disengaging carbonic acid, of which I have had proof in several trials.

Sulphuretted hydrogen does not expel carbonic acid from the alkalies in solution in water. Bicarbonate of soda dissolved to saturation in distilled water absorbs, I find, 143 per cent. of its volume of this gas; whilst the serum of blood (it was Sheep's that was tried) dissolved 207 per cent., arterial blood 235, and venous 290. This, too, is unfavourable to the same idea; as is also the large proportion of carbonic acid which blood, it has been shown, is capable of absorbing.

Supertartrate of potash occasions an effervescence, when mixed in substance with a solution of the sesquicarbonate, but not of the carbonate of soda; and the effect is similar, whether the mixture be made over mercury, air excluded, or in an open vessel exposed to the atmosphere. The supertartrate of potash also, I find, mixed with blood and agitated with common air, acts as with the sesquicarbonate, and occasions a disengagement of air, and both from arterial and venous blood, and from serum; and the air I have ascertained is carbonic acid.

From these facts, may it not be inferred that the alkali in the blood, in its normal or healthiest condition, is neither in the state of carbonate nor of bicarbonate, but of sesquicarbonate? The power of the blood to absorb carbonic acid and sulphuretted hydrogen accords best with this view, and some other important properties of the fluid are, I believe, in harmony with it.

The sesquicarbonate, I may add, seems to be the state of rest of the alkali in combination with carbonic acid, under ordinary circumstances of exposure to the atmosphere. Thus the native compound is the sesquicarbonate, as is also, I believe, the effloresced salt*. And I find that although a solution of the bicarbonate may be brought by the air-pump to the state of sesquicarbonate, it cannot be reduced to that of the carbonate: after it has ceased to give off any air in vacuo, it effervesces

* On exposing carbonate of soda in excess to carbonic acid gas over mercury, the gas is rapidly absorbed with the expulsion of part of the water of crystallization, so as to produce an appearance of deliquescence, and the sesquicarbonate is formed.

with the supertartrate of potash; and if evaporated to dryness over sulphuric acid under an exhausted receiver, on being subjected to heat, it disengages carbonic acid gas.

IV. *Does the blood contain any gas capable of being extricated by the air-pump?*

On this subject also there has been much difference of opinion. Our distinguished countryman Mayow, more than a century ago, stated that blood, especially arterial blood, effervesces in vacuo, which he attributed to the disengagement of air, his Spiritus Nitro-aëreus*. Sir Everard Home, on the authority of Mr. Brande, in 1818, asserted that blood, both venous and arterial, under the exhausted receiver evolves a large quantity of carbonic acid gas, an ounce of blood as much as two cubic inches of gas†. I repeated the experiment shortly after, but without confirming the result; neither by the air-pump, nor by heat applied even to coagulation of blood and serum in close vessels, did I succeed in demonstrating the extrication of this acid‡. Since that time the experiment of the air-pump on the blood has been frequently made, and by observers of great accuracy, as by Drs. Duncan and Christison in this country, by MM. Tiedeman, Gmelin, Mitschenlich and Müller on the continent, and recently by MM. Bischoff and Magnus. With the exception of the last-mentioned inquirers, the results have been negative. MM. Bischoff and Magnus, on the contrary, state that by careful exhaustion they have obtained gas from the blood; the former a small quantity of carbonic acid gas, the latter a notable quantity, and not only of carbonic acid gas, but also of oxygen and azote§.

M. Magnus attributes the failures of former experimenters to their having used pumps of imperfect exhausting power, or to their not having carried the exhaustion sufficiently far. In my early and first trial I employed the air pump belonging to the laboratory of the Royal Institution, which was an excellent one, and which I then believed was in good order; but I might have been mistaken.

To endeavour to satisfy myself on this point, I have had an air-pump constructed for the purpose, under the direction of an able artist, Mr. Ross, of 33 Regent Street, already distinguished for his excellent achromatic microscopes, and it has answered perfectly. When the exhaustion has been carried as far as possible, the difference of level of the mercury in the siphon-gauge has not exceeded a quarter of an inch, and over water has not exceeded half an inch.

Experimenting with this machine on blood, collected with such precautions as I believe to have been adequate to insure accuracy of results, in a majority of instances the disengagement of gas has been rendered manifest, and both from arterial and venous blood.

* Johannis Mayow Opera omnia. Hagæ Com. 1681. p. 133.

† Philosophical Transactions for 1818, p. 181.

‡ Ibid. 1823, p. 516.

§ Annales des Sciences Nat. tom. viii. p. 79. *et seq.* contain a translation of M. Magnus' paper, with a figure of the apparatus, &c. used; the original appeared in Poggendorf's Journal, vol. xl. part 3.

I shall briefly mention the trials which I consider most conclusive, and which satisfied me in spite of an opposite pre-existing bias.

Vials provided with well-ground glass stoppers were filled with distilled water, deprived as much as possible of air by the air-pump; they were then placed under the receiver and kept in vacuo until all adhering air was removed; the stoppers were now introduced, all air being excluded, and they were instantly immersed in distilled water, which had been well boiled. Thus prepared they were taken to an adjoining slaughter-house, where they were filled with Sheep's blood in the following manner, without its coming in contact with the air. For venous blood the jugular vein was exposed; two ligatures were applied to it; the vein was divided between the two, and the upper part, slightly detached, was introduced into a prepared vial under water the instant the stopper was withdrawn, and laid open. The heavier blood proceeding from the vessel of course expelled the water; and when it was supposed to be all expelled the stopper was restored, and the vial was replaced in the water. In the instance of arterial blood it was collected in the same manner from the carotid artery. In some trials the blood was allowed to coagulate undisturbed; in others the fibrin was detached, and the liquidity of the blood preserved by agitating it, the instant the stopper was replaced, with some mercury, introduced with the distilled water, and equally deprived of adhering air.

In about half an hour from the abstraction of the blood, in every instance, it was subjected to the air-pump. The instant the stopper was withdrawn the vial was placed in a small receiver on the plate of the pump, and covered with a little larger receiver, and the air as soon as possible exhausted. No appearance of disengagement of gas was perceptible until the exhaustion was nearly complete; then it was sudden, sometimes considerable, even to overflowing, in the form of bubbles, and it continued some time. The results were not distinctly different, that I could perceive, whether venous or arterial blood was used; I am disposed to think, on the whole, that less air was disengaged from the arterial blood than from the venous.

When blood allowed to coagulate in the vessels was tried, the results varied a little, and appeared to me instructive. At first, on exhaustion, only a few particles of air were disengaged; no more, it might be supposed, than were derived from the contact of the end of the stopper. In two experiments such was the appearance for at least five minutes, conveying the idea that no air was extricated; then abruptly a bubble or film burst with some force, as was denoted by the scattering of the particles of blood; and a bubbling commenced and continued, rendering the indications of extrication of gas unquestionable. And in conformity with this result I may remark, I have never succeeded in obtaining indications of air in the blood in operating on it by the air-pump, if confined in a detached portion of vein, or in the heart of animals, the great vessels, previous to excision, having been tightly secured by ligature. In no instance of this kind have I witnessed any distention, such as occurs if air be admitted previous to the application of the ligatures: clearly indicative, it appears to

me, that a very slight compressing force is sufficient to confine the air in the blood, or rather, I should say, prevent its substance assuming the elastic state; and further, the probability, that the quantity of air so condensed is small.

I have stated that in the majority of instances the indications of the disengagement of air from blood in vacuo have been manifest. Exceptions, however, have occurred, and those clear and decisive; inducing me to believe that the quantity of air condensed in the blood is variable; that there are times when it is in excess, and times when in deficiency, and when totally wanting, connected with regularly changing states of the functional system. I hope on another occasion to be able to recur to this part of the subject, on which at present I have collected but a few facts. I may add, that such facts tend in part to reconcile some of the discrepancies referred to in the beginning of this section.

V. *Of the air or gases contained in the blood capable of being extricated by the air-pump.*

As already mentioned, M. Magnus has stated that these gases are carbonic acid, oxygen and azote in notable quantities. Taking the mean of ten of his experiments on the blood of the Horse and Calf (five on arterial and five on venous), the total quantity of the mixed gases he obtained was, in the instance of arterial blood, 10·4 per cent. per volume, and in that of venous 7·6 per cent.; the former consisting of about

6·5 carbonic acid,
2·4 oxygen,
1·5 azote;

the latter of

5·5 carbonic acid,
1·1 oxygen,
1·0 azote.

On a subject of so much importance, it is very desirable that the experiments of M. Magnus should be repeated and verified; for until this is done, considering the physiological history of the blood, it will be difficult to avoid doubt, and to depend on them with that degree of confidence which is justly due only to well-authenticated facts.

The ingenious apparatus employed by M. Magnus being difficult of construction, and not easily used excepting in a well-appointed laboratory, I must leave the repetition of his highly interesting experiments to those inquirers who are more happily situated than myself for engaging in them. On the present occasion I shall limit myself to the detail of some experiments instituted with a view of testing M. Magnus' general results.

As a solution of potassa has the property of absorbing carbonic acid gas, it follows that if mixed with the blood previous to being subjected to the air-pump, it will prove in some measure a test of the kind of air which the blood is capable of affording.

With this intent two vials were prepared, the same as before used, one filled with distilled water, the other with a weak aqueous solution of caustic potash, both care-

fully deprived of air by the air-pump. Observing the same precautions as before, a portion of venous blood from a Sheep was received into each of them. When less than half of the water and of the solution, as well as could be guessed, was expelled, the vials were closed with the glass stoppers belonging to them, and instantly immersed in water, and as soon as possible subjected to the air-pump. The results of exhaustion in the two instances was perfectly distinct. From the blood mixed with water gas was disengaged; there was a continued ascension of bubbles. From the blood mixed with the alkaline solution no gas was liberated, excepting a bubble or two, which might fairly be considered as entangled air derived from contact of the blood with the stopper.

A similar comparative trial was instituted with arterial blood of the Sheep, and the results also were similar and equally well marked.

Considered as test experiments, the first inference from these results is, that carbonic acid gas, or an air absorbable by a solution of potash, is disengaged both by venous and arterial blood in vacuo; and next, that no other gas is disengaged from either of them, neither oxygen nor azote, each of which is unabsorbable by the solution in question. I have repeated the experiment twice on the venous blood of Man, and twice on the venous blood of the Sheep, and twice on the arterial blood of the latter, without variation of results; and they are more to be depended on, as the alkali has the effect of preserving the blood liquid.

I may mention another method which I have employed as a test experiment; and first in relation to carbonic acid. If the blood contain a notable portion of this gas capable of being extricated by the air-pump, it necessarily follows, that when subjected to the action of the air-pump, and deprived as far as possible of its fixed air by this means, it will be capable of absorbing a larger quantity of carbonic acid than previous to the exhaustion. The following Table contains the results of three comparative trials on the venous blood of the Sheep, its fibrin separated or detached in the usual manner.

Table showing the absorption of carbonic acid gas by

Venous blood subjected to the air-pump.			Venous blood not subjected to the air-pump.		
Volume of blood used.	Volume of carbonic acid introduced.	Volume of gas absorbed.	Volume of blood used.	Volume of gas introduced.	Volume of gas absorbed.
27	46	38	27	47	37
27	45	39	27	45	39
27	46	35	27	45	34

Although I have thought it right to notice these results, and although they are in accordance with the preceding, I do not attach value to them, excepting as tending to show that the quantity of carbonic acid gas extracted by the air-pump, when the blood affords it in vacuo, is small.

The same mode of reasoning suggested comparative trials of the absorbent power

of arterial blood for oxygen, before and after exhaustion by the air-pump, as a further test experiment, whether arterial blood contain oxygen in a free state, that is, admitting of being extricated by the removal of atmospheric pressure. The result of this trial also has been negative; its power of absorbing oxygen has not appeared to be at all increased by exhaustion; this at least was the result of one experiment carefully conducted.

VI. *Is any oxygen contained in the blood not capable of being extricated by the air-pump?*

Before I was acquainted with the researches of M. Magnus I had instituted some experiments to endeavour to determine whether any oxygen in a free state, or in a condition approximating to that state, exists in the blood, and especially in the arterial, admitting of being detected by means of substances possessing a strong attraction for oxygen, or of being expelled by substances of greater solubility in blood than oxygen. Hydrogen, phosphuretted hydrogen, sulphuretted hydrogen, nitrous oxide and nitrous gas, it appeared advisable to try, as belonging to the first class of substances, and carbonic acid gas as belonging to the latter.

The results with hydrogen, sulphuretted hydrogen, nitrous oxide and carbonic acid gas, were of a negative kind. Neither using arterial blood, nor blood which had been agitated with oxygen, and which had absorbed or made to disappear a certain quantity of this gas, could I detect any indications of its presence either by combination or expulsion. In the instances of nitrous oxide and carbonic acid, however, it may be worthy of remark, that the blood which had been agitated with oxygen absorbed less of either of these gases than it did before it was so treated*.

The results with phosphuretted hydrogen, the spontaneously inflammable species, were of an ambiguous kind, not sufficiently clear to deduce from them any satisfactory conclusion. In one trial, serum of the venous blood of the Sheep absorbed nine per cent. of this gas; venous blood 11·3 per cent.; and arterial 5·8 per cent†.

The results with nitrous gas were of a different kind, and may be deserving of being specially noticed.

The blood used was that of the Sheep prepared in the usual manner. The experiments were made during the very cold weather which prevailed in the beginning of the year, and the difference of colour between the venous and arterial blood was very strongly marked.

* Nitrous oxide I find is absorbed in about the same proportion by venous and arterial blood, and by the serum of blood, and also in about the same proportion by water. Thus, at the temperature of 45° over mercury, using the blood and serum of the same animal (the Sheep), thirty-two measures of each absorbed twenty-two measures of this gas, and thirty-two of distilled water absorbed 21·5.

† Supposing the gas decomposed, the phosphorus uniting with the oxygen in the blood, the apparent smaller absorption by arterial than by venous blood is what might be expected, on the idea that the former kind of blood contains most oxygen.

1st. On Arterial Blood.

1. Fifty-three measures of this blood were agitated over mercury with forty-six measures of nitrous gas; there was a diminution of volume of seventeen measures.

2. Fifty-three measures of the same blood (another portion) were agitated with nine of oxygen; two measures were absorbed.

3. Fifty measures of this blood, so treated with oxygen, were agitated with forty-seven of nitrous gas; there was a diminution of twenty-two measures.

2nd. On Venous Blood.

1. Fifty-three measures of this blood were agitated with fifty of nitrous gas; there was a diminution of ten measures.

2. Fifty-three measures (another portion) were agitated with ten of oxygen; five measures were absorbed.

3. Fifty-one measures of blood so treated were agitated with forty-nine of nitrous gas; there was a diminution of seventeen measures.

The residual air in each instance was examined and was found to be a mixture of nitrous gas and azote without carbonic acid gas. The azote, it may be presumed, was introduced with the nitrous gas; it was in the same proportion as that which adulterated it, viz. about four per cent. That the residual air was free from carbonic acid was inferred from the circumstance, that in comparative experiments with and without addition of a portion of solution of caustic alkali, there was no difference in the proportion of nitrous gas absorbed: and it was corroborated by another circumstance, viz. that after the absorption of the nitrous gas, the blood was capable of absorbing seventy-five per cent of carbonic acid gas*; and further by the result that when nitrous gas is added not to saturation, the whole of it is absorbed†.

As regards the blood itself, the colour of both venous and arterial was altered; both were rendered darker and browner, as if a minute quantity of nitric acid had been added to them, a change long known to be occasioned by nitrous gas. In the degree of change there was however a difference; in the instance of arterial and of oxygenated arterial blood, it was more strongly marked than in that of the venous.

In conjunction with the unsuccessful attempts with the other gases already mentioned, do not the results just described indicate that a portion of oxygen exists in

* To some venous blood of a Sheep which absorbed 182 per cent. of carbonic acid gas, so much of a solution of pure hydrate of potash was added, that it absorbed 218 per cent. of the acid gas; seventeen measures of this blood with excess of alkali, agitated with fifty-one of nitrous gas, absorbed 5·5 measures; sixteen of the blood without the excess of alkali absorbed five measures: thirty-four measures of carbonic acid gas were added to the latter, the excess of nitrous gas being left in the tube; on agitation twelve measures of the carbonic acid were absorbed.

† In one experiment eighty-four measures of the venous blood of the Sheep were agitated with ten of nitrous gas over mercury; the whole of the gas was absorbed: twelve of oxygen were added; on agitation again there was no further absorption.

the blood, not capable of being extracted by the air-pump, and yet capable of entering into combination with nitrous gas, and which exists in largest proportion in arterial blood? Unless this conclusion is adopted, it must be supposed either that the nitrous gas which disappears is decomposed, or that it combines directly with the red particles; neither of which suppositions is well supported by facts. The circumstance that no azote is disengaged, is not favourable to the idea that the nitrous gas is decomposed; and the difference of effect in the instances of venous and arterial blood, and, after and before agitation with oxygen, is not in accordance with the notion of direct combination. I may mention another fact which seems to have the same bearing: serum, which does not absorb oxygen, I find also does not absorb nitrous gas, excepting in about the proportion in which water absorbs it: and, further, in corroboration, I may mention, that as blood putrefies, whether arterial or venous, its power of absorbing nitrous gas diminishes; and that it is also diminished by being agitated with phosphuretted hydrogen. Thus the arterial blood of a Sheep, which before agitation with phosphuretted hydrogen absorbed 45·3 per cent. of nitrous gas, after agitation with it absorbed 7·4 per cent. less; and, after it had become putrid, it absorbed twenty per cent. less. According to my observations, arterial blood does not lose its peculiar florid hue under the action of the air-pump. Is not this also in favour of the above inference, that a portion of oxygen is retained by the blood resisting extraction by the air-pump? I find also that when venous blood is agitated with oxygen and subjected to the air-pump, it, in like manner, retains its acquired florid vermilion hue, and likewise a power of absorbing an additional quantity of nitrous gas.

VII. *When oxygen is absorbed by the blood is there any production of heat?*

To endeavour to determine this point, of so much interest in connexion with the theory of animal heat, a very thin vial, of the capacity of eight liquid ounces, was selected and carefully enveloped in bad conducting substances, viz. several folds of flannel, of fine oiled paper, and of oiled cloth. Thus prepared, and a perforated cork being provided, holding a delicate thermometer, two cubic inches of mercury were introduced, and immediately after it was filled with venous blood, kept liquid as before described. The vial was now corked and shaken; the thermometer included was stationary at 45°. After five minutes that it was so stationary, the thermometer was withdrawn, the vial closed by another cork was transferred inverted to a mercurial bath, and 1½ cubic inch of oxygen was introduced. The common cork was returned, and the vial was well agitated for about a minute; the thermometer was now introduced, it rose immediately to 46°, and continuing the agitation it rose further to 46·5, very nearly to 47°. This experiment was made on the 12th of last February on the blood of the Sheep.

On the following day a similar experiment was made on the venous blood of Man. The vial was filled with eleven cubic inches of this blood, its fibrin broken up in the usual manner, and with three cubic inches of mercury; the temperature of the blood

and mercury was 42·5, and the temperature was the same after the introduction of three cubic inches of oxygen. The temperature of the room being 4[illegible]°, a fire having shortly before been lit, the vial was taken to an adjoining passage where the temperature of the air was 39°. Here the vial was well agitated, held in the hand with thick gloves on as an additional protection. After about three quarters of a minute the thermometer in the vial had risen a degree, viz. to 43·5.

On the 14th of the same month a third experiment was made on venous blood from the jugular vein of a Sheep. The vial was filled with 3·5 cubic inches of mercury and eleven cubic inches of blood. The thermometer in the bottle, left five minutes, was stationary at 49°; the temperature of the mercurial bath was 49°; the air of the room was 52°; a thermometer with its bulb moistened was 45°, which I mention because the outer covering of the vial was moistened with some blood which had overflowed. After three cubic inches and a half of oxygen had been introduced, before agitation, the thermometer was still 49°. The bottle was briskly shaken for about half a minute; now, on observing the thermometer, it was found at 50°; the vial was again agitated; there was no further increase of temperature: after ten minutes it had fallen to 49°.

I shall relate one experiment more, and that on arterial blood. It was made on the 14th of February, and in the same manner as those on the venous blood. Before and after the introduction of the oxygen, the blood, which was from the carotid artery of the Sheep, was 45°; after agitation with oxygen it rose to 45·5: this was done when the temperature of the air was 39°.

In a former part of this paper I proposed to recur to the question, Is the fixation of oxygen in the blood attended with the formation of carbonic acid gas? The change of colour accompanying the fixation of oxygen by the blood, so different from that produced by carbonic acid, and the effect of nitrous gas before and after, seem to be most in favour of the idea, that the oxygen, in the first instance, is simply absorbed, and that the heat evolved is merely the effect of its condensation; or, that if any of it enters into immediate union with the carbon, it is only a small part of the whole.

VIII. *Conclusion.*

Should the results detailed in the preceding pages be confirmed on repetition, they can hardly fail having some effect on the theory of respiration and animal heat.

As regards the former, they appear to me to tend to show that the lungs are absorbing and secreting, and perhaps exhaling organs, and that their peculiar function is to introduce oxygen into the blood and separate carbonic acid from the blood.

As regards animal heat, they appear to favour the idea, that it is owing, first, to the fixation or condensation of oxygen in the blood in the lungs in conversion from venous to arterial; and secondly, to the combinations into which it enters in the circulation in connexion with the different secretions and changes essential to animal life.

In illustration of what I imagine the secreting power of the lungs, I may mention the difference of effects in an instance of death by strangulation, and another by ex-

haustion of air from the lungs by the air-pump. A full-grown Guinea Pig was the subject of experiment in each. The one killed by strangulation died in about a minute after a cord had been drawn tightly round its neck; the other, placed on the plate of the air-pump and confined by a receiver just large enough to hold it, lived about five minutes after the exhaustion had been commenced, the pump the whole time having been worked rapidly. The bodies were immediately examined. The heart of the strangled animal was motionless; it was distended with dark blood; twelve measures of the blood, broken up and agitated with twenty-nine of carbonic acid gas, absorbed eighteen measures, or 150 per cent. The heart of the other Guinea Pig was also distended with blood, but of a less dark hue. Its auricles were feebly acting; the lungs were paler than in the former and more collapsed: ten measures of blood from the heart, broken up and agitated with fifty of carbonic acid gas, absorbed thirty-seven measures, or 370 per cent.!

Further, in illustration of this supposed secreting power of the lungs, I might adduce the condition of the blood in disease, and in instances in which I have examined it after death from disease, in the majority of which I have found the blood loaded with carbonic acid, as indicated both by the disengagement of this gas, when the blood was agitated with another gas, and by the comparatively small proportion of carbonic acid which the blood was capable of absorbing. This condition of the blood, in relation to carbonic acid, I believe to be one of great interest and importance, and capable, when further investigated, of throwing light on many obscure parts of pathology, and especially on the immediate cause of death, and that happy absence of pain in dying which is commonly witnessed.

As regards an exhaling power, which I suppose the lungs may possess, I conceive it may be exercised occasionally under peculiar circumstances—circumstances, in the first instance, favouring an accumulation of carbonic acid gas in the blood, as undue pressure of any kind, and, in the second instance, circumstances of a different nature, connected with the removal of undue pressure, admitting thereby the excess to pass off.

The view which I have alluded to relative to the production of animal heat, is, I believe, capable of explaining very many particulars of animal temperature in different classes of animals, and both during life, in health, and disease, and in a state of hybernation and after death. If correct, this it must necessarily do, theory being merely an expression of facts and truths in nature being perfectly consistent.

Fort Pitt, Chatham,
May 30th, 1838.

ERRATUM.

Page 288, line 28, *for* $\frac{1}{100}$ *read* $\frac{14}{100}$.

XV. *Researches in Embryology. First Series.* *By* MARTIN BARRY, *M.D. F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh. Communicated by* P. M. ROGET, *M.D. Sec. R.S.*

Received June 20,—Read June 21, 1838.

IT has been truly said, that "in all the sciences of observation, the great difficulty generally consists in taking the first steps." A hundred and fifty years have now elapsed since the celebrated REGNER DE GRAAF, after a series of well-conducted observations, maintained that the ovum of the Mammalia must exist already formed in the ovary; an opinion which, after meeting with violent opposition, appears to have been nearly abandoned, and superseded by the notion countenanced by HALLER, that the ovum was formed in the Fallopian tube out of a substance discharged from the ovary. A century after DE GRAAF had promulgated his opinions, CRUIKSHANK arrived at the same conclusion, that the ovum was really formed in the ovary; but he sought it there in vain. PREVOST and DUMAS in 1824 obtained a glimpse of something that must have been the ovum in that organ; VON BAER in 1827 found and recognised it there.

This important discovery of BAER formed an epoch in the history of development; but it was a "first step," and the object one of extreme minuteness. It was therefore not surprising if the excellent discoverer did not see or justly estimate all that appertained thereto; and he said himself "there remains yet many a thing that will become a prize" for others.

VON BAER for instance did not see the germinal vesicle contained within the mammiferous ovum; he saw no more than a transparent space. This, however, was an oversight of the first importance, because that which gives peculiar interest to the germinal vesicle is the fact, now generally acknowledged, that it is the most essential portion of the ovum; and besides, as in the following pages I shall have to show, this structure and its contents are the earliest that appear in the order of formation. Overlooking the germinal vesicle in the Mammalia, VON BAER supposed the ovum itself to represent, in that class of animals, the germinal vesicle of Birds†. Other analogies, and they are not few, which he based on this were consequently erroneous.

I have thought it proper to make these introductory remarks, because of the neces-

† Professor BAER called the vesicle he discovered in the ovary, not the *ovum* but the "*ovulum*." If, however, it can be made to appear extremely probable that the chorion or external membrane of the ovum of the uterus is a primitive part of the ovarian vesicle of BAER, it is perhaps better to call the latter an *ovum*, as will be done in this memoir.

sity that lies before me, in offering the present contribution in embryology, for pointing out several other structures connected with the discovery of Baer, the nature of which I believe this author to have mistaken, and some that appear to have escaped his notice. This I shall do with great deference, asking his re-inspection of the parts, and respectfully soliciting an examination of them by other physiologists who have been engaged in researches on the early stages of the ovum of the Mammalia, especially Professors Purkinje, Krause and Coste; but more particularly Professor Valentin and my valued friend Professor Rudolph Wagner.

In the course of this investigation, which occupied many months, the number of individual animals examined was very large, furnishing me an opportunity for verifying most of the facts by repeated observation. The examinations were generally made very soon after death; and the drawings having been all taken by myself, I have it in my power to say that their fidelity may be relied upon†.

In the year 1837, preparatory to the commencement of my own researches, I spent some time in Germany for the purpose of becoming acquainted with the known facts on animal development and other objects of microscopic research; and cannot omit this opportunity of gratefully acknowledging the facilities afforded me on that occasion by my honoured friends Professors Johann Müller, Ehrenberg, Rudolph Wagner, and Dr. Th. Schwann. The microscope I use is an achromatic, since made for me by Schiek of Berlin, and similar in all respects to the instrument employed by Professor Ehrenberg.

It may not be improper in the first place to furnish an idea of what has been already published on some branches of the subject; for it is one to which the attention of physiologists in this country has scarcely begun to be directed.

Valentin gives the following comprehensive description of the vesicles of Graaf:—

"In the ovary of each of the Mammalia, there is found a greater or less number of spherical pellucid vesicles, the so-called *folliculi Graafiani*, (Graafian vesicles,) the greater part of which are situated near the surface of the organ. Their size is very different, as well in different animals as in the same ovary of the same animal, since the older ones are sometimes four to five times, as in Rabbits, Dogs, Cats; sometimes eight to ten times, as in Man; sometimes ten to twenty times, as in ruminating animals; sometimes thirty to fifty times, and still more, as in the Swine; larger than those recently formed, and this, too, independently of their increase in volume after impregnation. When situated close to the surface, they are covered by the peritoneum alone; when deeper, by the latter, and by the fibrous tissue of the ovary, and closely invested by a net-work of vessels, between which there is a granular membrane. They are everywhere closed, without a trace of processes, but in-

† Some of the figures to be seen to advantage should be viewed with a large lens.

timately united with the substance of the ovary, so that in general it is not so very easy to dissect the vesicle freely out on all sides without laceration†."

Plate VII. fig. 62‡ presents the parts which Baer considers to enter into the formation of a Graafian vesicle. For an explanation of this figure I refer to the description of the plate. It appears to me however, as I shall endeavour presently to show, that a "Graafian vesicle" is *an ovisac that has acquired a covering proper to itself*, and as such the term will be used in the present paper.

The figure just referred to, exhibits the situation of the "ovulum" (ovum) of Mammalia, *according to* Baer, surrounded by its so-called "disc" within the Graafian vesicle. It will be shown in this memoir that there is no structure in the Graafian vesicle of a *discoid* form; and the error of Baer in mistaking the "ovulum" (ovum) of Mammalia for a part corresponding to the germinal vesicle of other animals, appears to be attributable in part to his misconception of the situation of the "ovulum" (ovum) in reference to its surrounding granules.

Purkinje§ questioned the analogy which Baer had imagined between his "ovulum" (ovum) of the Mammalia and the germinal vesicle of other animals; and the discovery at length by Coste in France, and by Valentin and Bernhardt in Germany, of a germinal vesicle in Mammals, showed the justness of Purkinje's reasoning, increasing the analogy between the bodies expelled respectively from the ovaries of Mammalia and those of other vertebrated animals. It will presently be shown that a perfect analogy does not exist between these bodies.

One of the last additions made to our knowledge in embryology, exceeding in minuteness all the rest, is the discovery by Professor Rudolph Wagner‖, on the internal surface of the germinal vesicle, of the *macula germinativa*, or germinal (germinative) spot.

The object of this memoir is to add some discoveries of my own on the early stages of the ovum, not only of the Mammalia, but of the other Vertebrata. This cannot be done except by means of details both minute and, I fear, fatiguing; but perhaps I may venture to hope for the reader's indulgence when he recollects the precision requisite in treating a question comparatively new in the history of physiology. The perusal would, however, be much facilitated by occasional reference to the following tabular Synopsis of the parts to be described. It might also be advantageous to examine the plates (V. to VIII.) in connexion with this table; for as the same letters denote the same objects in all the figures, and as these letters have been introduced

† Translation from the German, in the Edinburgh Medical and Surgical Journal, No. 127; which contains the early part of Professor Valentin's elaborate work "Handbuch der Entwickelungsgeschichte des Menschen mit vergleichender Rücksicht der Entwickelung der Säugethiere und Vögel," Berlin, 1835.

‡ Taken from Baer, Lettre sur la Formation de l'Œuf, translated by Breschet, Paris, 1829. fig. ix. (I have not the original at hand.)

§ Encyclopädisches Wörterbuch, Band x. Artikel "Ei," pp. 124—128, Berlin, 1834.

‖ Lehrbuch der Vergleichenden Anatomie, S. 351, Leipzig, 1834–5. Edinburgh Medical and Surgical Journal, No. 127. Prodromus Historiæ Generationis Hominis atque Animalium, Lipsiæ, 1836.

into the table, a reference to the more particular explanation of the plates might thus in a great measure be spared.

Synopsis showing the Order of Formation and the relative Position of the several Parts of the unimpregnated Ovum and the Graafian Vesicle of Mammalia, according to the author's observations.

First.	Second.	Third.	Fourth.	Fifth.	Sixth.		Seventh.
Fluid, *a*.	Fluid.	Fluid, *a*.	Fluid, *a*.	Fluid, *a*.	Fluid, *a*.		Fluid, *a*.
	Germ. spot.	Germ. spot, *b*.	Germ. spot, *b*.	Germ. spot, *b*.	Germ. spot, *b*.		Germ. spot, *b*.
Germ. vesicle, *c*.	Germ. ves.	Germ. ves. *c*.	Germ. ves. *c*. Oil-like globules (*).	Germ. ves. *c*.	Germ. ves. *c*.		Germ. ves. *c*.
Granules (*g*) and oil-like globules (*) †.	Granules (*g*) and oil-like globules(*).	Granules (*g*) and Fluid.	Granules (*g*) and Fluid.	Oil-like globules (*), Minute gran., and Fluid.	Yelk, *d*.	At this period the ovum is conveyed from the centre of the Graafian vesicle to its periphery.	Yelk, *d*.
				Memb. vit. *e*.	Memb. vitelli.		Memb. vitelli.
	Ovisac, h.	*Ovisac, h*.	*Ovisac, h*.	Chorion, *f*.	Chorion, *f*.		Chorion, *f*.
				Granules (*g*) and Fluid.	*Tun. gran.* g^1.		*Tun. gran.* (g^1) *and appendages.*
				Ovisac, h.	*Retinac.* g^2.		*Retinac.* (g^2) *and Memb.*
					Fluid.		Fluid.
					Memb. gran. g^3.		Memb. gran. g^3.
					Ovisac, h. *Tunic, i.* } = Graaf. ves. *h i*.		Graaf. ves. *h i*.
Plate V. figs. 1, 2, 3.	Plate V. fig. 9. &c.	Plate V. fig. 10. &c.	Plate V. fig. 14. &c.	Plate V. fig. 17.	Plate VII. fig. 50. &c.		Plate VII. fig. 55. &c.

The Table viewed *from left to right*, presents successive stages of Formation. The parts inserted in the same Column exist at the same time. Small print denotes such parts as, though existing, are obscured.

Viewed *from above downwards*, the Table shows the order of Position of the parts, proceeding outwards from the centre of the ovum; the innermost being marked *a*, the next *b*, the third *c*, and so on.

Italics are used in cases where the part has not been hitherto described, or where the name employed is new.

† The term "oil-like," occurs frequently in this memoir. It is not intended to describe the nature of the globules to which it is applied, but simply their appearance.

PART I.

On the origin and structure of the true Ovisac, a vesicle common to all vertebrated animals, but hitherto described as the inner membrane of the "Folliculus Graafianus" in Mammalia, and by some authors denominated the "chorion" in other Vertebrata; on the real nature of the "Folliculus Graafianus," and its relation to the calyx of the Bird; on the Germinal Vesicle and its contents, as the most primitive portion of the ovum; on the order of formation of the several other parts of the Ovarian Ovum; and on the true Chorion of Mammalia as a structure superadded within the ovary.

1. The early structure of the ovisac in the Mammalia may be seen in either quite young animals, or in those that have lately reached the state of puberty, when this vesicle and its contents are in the full vigour of formation. To find it in a young animal, I am in the practice of placing under the microscope thin slices cut from the surface of the ovary; or in animals after puberty, the outer portion of a large Graafian vesicle. Compression is of course required, but it should be very gradually applied. Great pressure is rarely wanted, and for the most part very undesirable.

The Ovisac of the Mammalia.

2. This vesicle originates in the proper substance (*stroma*) of the ovary†. Its general appearance when first formed is as a minute, pellucid, often yellowish sac, having an elliptic form, and plaits or folds in its contour (Plate V. figs. 4. to 12. *h*.). The ovisacs are found in groups, and those belonging to the same group are often of nearly the same size (figs. 4. 10. *h*.), though this is by no means constantly the case. Sometimes a group occupies more than the whole field of view; some of the ovisacs, more deeply imbedded than the rest, having the appearance of obscure pellucid spaces, others being superficial and distinct (fig. 4. *h*.). Could a fragment of amygdaloidal rock be made translucent and its amygdaloidal portions luminous, it would present an appearance not unlike that of a collection of minute ovisacs in the parietes of a Graafian vesicle from one of the Ruminantia. In Carnivora, such as the Dog and Cat, I have met with them having less of interstitial substance, innumerably heaped together (Plate V. figs. 6. 10., Plate VIII. fig. 68. *h*.). The surface of an ovary presents to the naked eye perhaps ten, twenty, or fifty Graafian vesicles, while the microscope discloses myriads; so that some parts bear no mean resemblance to the roe of fishes.

3. The ovisac at its origin, as just said, has an elliptic form. As its size advances it becomes more spherical, and is often met with somewhat tapered at one end (Plate VIII. fig. 74. *h*.). When it appears round in an early stage, this is perhaps owing to its being transversely situated under the compressor.

† It appears sometimes to originate in the parietes of a Graafian vesicle. Ovisacs met with in this situation will be more particularly referred to hereafter as *parasitic* ovisacs.

4. Its size when first formed is exceedingly minute. I have found it in several orders of Mammalia measuring not more than the 50th of a Paris line† in length. Such is the case, for instance, in the Ox (Plate V. fig. 4. *h*.); so that a cubic inch would contain upwards of two hundred millions, not merely of the elements of the ovum, but of the *ovisacs* of this animal. The minuteness of these vesicles indeed is almost incredible. In the Dog I have seen them measuring only the 100th of a Paris line‡ (Plate V. fig. 9. *h*.), thus little more than one third the long diameter of a blood-granule (red particle) of the *Proteus anguinus*§.

5. The ovisac is more or less pellucid according to its size; being most so in the early stages of formation, and becoming merely translucent as development advances. This is partly owing to the gradual addition of an external covering or tunic, to be hereafter more particularly mentioned (24. 25.). It continues however in all its stages more translucent than the substance in which it lies.

Cavity in which the Ovisac is often found.

6. More particularly considered, the situation of the ovisac in its early state is often found to be a cavity (Plate VIII. fig. 69.), sometimes, as already said, in the proper substance (*stroma*) of the ovary, and sometimes in the parietes of a Graafian vesicle. It lies loose in, and unconnected with the walls of, its containing cavity. That it does so is shown in the figure just referred to (fig. 69.), which presents an ovum, *f*, escaped from the ovisac, *h*, and lying external to the latter in the cavity. (This change in the situation of the ovum was seen to take place on the ovisac being burst under the microscope by means of the compressor; when the ovum became squeezed into an elliptic form. This has repeatedly occurred‖.) The cavity in which the ovisac lies, may sometimes be found after the latter has been pressed out of it.

The proper membrane of the Ovisac in Mammalia.

7. In ovisacs of the minutest size this membrane is perfectly transparent; yet in and near to its contour it looks as though it were "concentrically lamellar"¶ (Plate V. figs. 9. 11. 12. *h*.). I apprehend this appearance to arise from plaits or folds occurring in the membrane under pressure even at that early period, and indicating great susceptibility of distention. Its thickness is relatively very considerable in the smaller

† That is, about the 562nd of an English inch. See the table of measurements (118.), for a simple mode of reducing the fraction of a French line (‴) into the fraction of an English inch.

‡ = $\frac{1}{111}$ of an English inch.

§ Through the kindness of Professor R. Wagner I possess a living specimen of this animal, and have repeatedly confirmed the observation first made by him, that the red particles of its blood measure in some instances $\frac{1}{30}$ of a Paris line in length, and that they are visible with the naked eye, being larger than those of any other animal the blood of which has been examined.

‖ The ovum in this instance was ruptured before escaping, and left its germinal vesicle, *c*, behind.

¶ The appearance here referred to has been observed by R. Wagner, who uses the above expression in describing what he considers minute *Graafian vesicles* (Beiträge, &c., S. 28.).

ovisacs. The membrane of one of these, measuring in its long diameter $\frac{1}{75}$th of a Paris line†, had a thickness of $\frac{1}{600}$th of a line‡. When somewhat advanced in size, however, the thickness is sometimes relatively as well as absolutely greater. Yet so transparent is this membrane, that even the form of granules contained in its cavity may be tolerably well observed from the exterior (Plate V. figs. 9. 10. 11. 12. 17. *g*.); the degree of transparency varying with the pressure applied: and when great pressure has been removed, the previous state in this respect returns, and the vesicle resumes more or less completely its natural size, which indicates elasticity as well as susceptibility of distention. This membrane has a kind of undulating surface, presenting myriads of depressions or cells, to which is referrible the plaited or folded appearance of the contour, produced by pressure. Neither fibres nor granules being found in the texture of this membrane, it is probably without organization.

The peculiar Granules of the Ovisac.

8. The only objects which in general are *visible* (12.) in the ovisac when the latter is first formed, are a few granules (Plate V. figs. 8. 9. 11. 12. *g*.). These granules deserve a particular description, from their peculiar appearance, from their entering into the formation of several important structures to be described in Part II. of this memoir, and from their being very intimately connected with the evolution of the ovum from its early appearance as the germinal vesicle (14—22.), until as a mature ovum it has passed into the Fallopian tube.

9. These granules are elliptical or ellipsoidal, sometimes nearly round, and generally flattened (Plate VIII. fig. 73. *g*.). When lying closely together, their form becomes by pressure polyhedrous (Plate VI. fig. 44. g^1.). They are exceedingly transparent, yet often punctate, which latter appearance seems sometimes to arise from the presence of very minute oil-like globules. They present, with more or less distinctness, a nucleus; and I have met with two nuclei in a single granule. In the substance of the nucleus, when highly magnified, there is observable a point still darker (Plate VIII. fig. 73.). These peculiar granules sometimes disappear, apparently by liquefaction; preparatory to which change I have observed them to become more spherical and brightly pellucid, seeming to contain a fluid in their interior (Plate VI. fig. 44. g^1.). In size I have found them to vary from about the 400th to the 100th of a Paris line§, but they are often about the 200th of a Paris line‖ in length. As viewed with reflected light, they appear greyish white in colour. Water dissolves them.

10. This is a general description of these very peculiar granules. In some orders of the Mammalia I have found them more regularly elliptic or more round than in others; in birds I have thought them less regular in form; in certain animals they have a decided tinge of yellow: but in these respects, as well as in their punctate or subgranulous appearance, and in their stages of liquefaction, their condition

† = $\frac{1}{75}$ of an English inch.
‡ = $\frac{1}{600}$ of an English inch.
§ = $\frac{1}{400}$ to the $\frac{1}{100}$ of an English inch.
‖ = $\frac{1}{200}$ of an English inch.

differs at different periods in the same animal. Yet notwithstanding these varieties there is always found a general resemblance, so that when once examined these granules cannot fail to be recognised †.

The Fluid contained in the Ovisac.

11. This fluid is pellucid, often yellowish, partially coagulable, and is generally considered to be albuminous. It contains a large quantity of the peculiar granules just described, and a varying quantity of oil-like globules. This fluid having been already described by others ‡ I need not add more respecting it.

On the Germinal Vesicle and its contents as the most primitive portion of the Ovum.

12. It has appeared desirable to give a description of the ovisac the first place in this memoir, but there is a portion of the ovum which exists before it. Each of the many ovisacs already referred to, such as those in Plate V. fig. 4. *h*., and even the minutest of those in fig. 9. *h*., probably contained, besides the peculiar granules visible (8.) in their interior, a concealed part, which, supposed to be the most important, also appears to be the most primitive element of the ovum.

13. Purkinje, the first discoverer of the germinal vesicle in any animal (the Bird), having observed that its relative size was greater, the more minute its containing ovum, has expressed the opinion that it is the first part of the ovum formed §. Baer, the discoverer of this vesicle in other oviparous Vertebrata, as well as in Mollusca, Annelida, Crustacea, and Insects ‖, believes that he observed its formation in animals belonging to the two first-mentioned classes to precede the evolution of the ovum ¶. R. Wagner has described and figured the posterior extremity of the oviduct in insects (*Acheta campestris*) †† as filled with little besides germinal vesicles, each of which contains a spot; and in a later publication ‡‡ he has shown similar objects in one of the Libellulæ (*Agrion*), and also in the freshwater Beetle (*Dyticus marginalis*). These observations of Baer and Wagner I have been enabled to extend certainly to two, and I think to three classes of the Vertebrata.

14. Plate V. fig. 1., taken from the Rabbit, presents vesicles, *c*, containing fluid, each of which has its own envelope, consisting of the peculiar granules, *g*, just described (8.). These vesicles, of which a considerable number was seen in the same field of view, were for the most part elliptical, some of them nearly globular, their long diameter varying from the 150th to the 50th of a Paris line §§. The contained

† Several writers have made mention of these granules, but an adequate description of them I do not find to have been given.

‡ Baer, Lettre, &c., pp. 18, 19. Valentin, Handbuch der Entwickelungsgeschichte, &c., S. 15, 16. Bernhardt, Symbolæ ad Ovi Mammalium Historiam ante Prægnationem, pp. 10, 11; Vratislaviæ, 1834.

§ Encyclopädisches Wörterbuch, Band x. S. cxi.

‖ Burdach, Die Physiologie als Erfahrungswissenschaft, Band i. § 63. 1835.

¶ Lettre, &c., p. 21. †† Prodromus, &c., fig. xviii. *a*. ‡‡ Beiträge, &c., Tab. ii. figs. 1 to 6.

§§ = $\frac{1}{72}$ to $\frac{1}{144}$ of an English inch.

fluid did not appear perfectly pellucid, but presented an indistinct cloudiness. In one of the larger vesicles there was observed a well-marked spot (fig. 1. *b*.). The envelope of granules, *g*, was in all instances elliptical, its long diameter being about three times that of the vesicle it surrounded.

15. In the same Plate, figs. 18 and 19, from the Pigeon, are seen vesicles of the same kind containing fluid, some of them surrounded by oil-like globules, and others by granules of the same appearance as those in fig. 1, having mixed with them a few oil-like globules; the chief difference being that here the form of the whole object is not quite so regular as that of those in fig. 1. It will be observed that in fig. 18. the vesicles are surrounded by oil-like globules only (*); the envelope of peculiar granules seems to be subsequently added (fig. 19. *g*.).

16. In figs. 2. and 3, *g*, are presented elliptic masses of the same kind of peculiar granules. One of these masses (fig. 2. *g*.) measured $\frac{1}{75}$th, and the other (fig. 3. *g*.) $\frac{1}{150}$th of a line in length. These objects, met with in the Cat, appear to have been of the same nature as those just described from the Rabbit and the Pigeon, with this difference, that here the contained vesicle was not seen; the cause of which is perhaps referrible to oil-like globules having been mixed in large quantity with the peculiar granules. See figs. 2. and 3 (*). That these elliptic masses from the Cat (figs. 2. and 3.) corresponded to those from the Rabbit and the Pigeon, and that each of them contained a vesicle, is not indeed demonstrable; but their form, their size, and the nature of the granules render this extremely probable. I should add, as another reason for this opinion, that *patches* of these elliptic masses (figs. 2. and 3.) have been met with, sometimes in the state *g*, and sometimes in that marked (*). Similar objects have been seen in the Hog†.

17. I have found distinct vesicles in the Salmon, measuring in diameter the 50th of a Paris line‡, and less (Plate VI. fig. 30. *c*.), apparently corresponding to those from the Rabbit (Plate V. fig. 1. *c*.) and Pigeon (figs. 18. and 19. *c*.), but having more obscure contents§, and being destitute of a regularly formed envelope of granules.

18. I return to figs. 1, 18, and 19, Plate V., from the Rabbit and the Pigeon, and shall not refer to any besides these in drawing my conclusions. After an attentive examination of these objects, I do not think it hazardous to express my belief that they present one of the earliest stages in the formation of the ovum, the enveloped sac (*c*) being, as I suppose, the *germinal vesicle* itself.

19. The long diameter of this vesicle (*c*) in fig. 1, the stage now under consideration, varies, as already said, from the 150th to the 50th of a Paris line. It is not likely

† In one of the Mammalia I have seen vesicles filled with fluid, and apparently of the same kind as those in fig. 1, but without any peripheral accumulation either of granules or oil-like globules. These vesicles, however, were immediately lost from the field of view, and I have not since observed any in the same state.

‡ = $\frac{1}{111}$ of an English inch.

§ Which consisted of a fluid and minute oil-like globules, having a yellowish colour. Compare the vesicles in this figure with the vesicles (*c*) in fig. 35. from the same animal.

One of the vesicles (*c*) in fig. 30. has another vesicle external to it. (See Explanation of the Plates.)

that so great a difference in magnitude as this in the same animal should be permanent; and if this be admitted, it follows that after formation the germinal vesicle increases in its size. It may therefore when first formed be much minuter than the smallest of those which I have figured.

20. I find that the germinal vesicle of a mature ovum of the Rabbit does not generally exceed the 50th of a Paris line† in diameter; and this being just the size of the largest of those from the same animal in fig. 1, it seems probable that in the Rabbit‡ the vesicle does not increase much in magnitude after this early period.

21. It is proper to state, that in certain animals I have met with *ovisacs* much minuter than some of the objects (still uncovered by an ovisac) in fig. 1. This admits of explanation by supposing either that the germinal vesicle is much *smaller*, that its granulous envelope is much *thinner*, or that the period of formation of the ovisac, in reference to the size of the germinal vesicle, is much *earlier* in some animals than in others. There may also be a difference in different animals in each of these respects.

22. From the observations of Professors Baer and R. Wagner in invertebrated animals, and those now recorded from researches in two classes of the Vertebrata, I think I am warranted in concluding that the germinal vesicle and its contents constitute throughout the animal kingdom the most primitive portion of the ovum. In Birds and Mammalia the succeeding process appears to consist in the accumulation around the germinal vesicle of oil-like globules and peculiar granules. See the first column in the Table, page 304.

The manner of Origin of the Ovisac.

23. Around the elliptic envelope of granules and oil-like globules just described as proper to the germinal vesicle of Mammalia and Birds in an early stage, I find that there is formed a membrane, seen in Plate V. fig. 20. *h*. If the contents of this membrane, which is still incomplete, be compared with the granules (*g*) in fig. 19, these objects will be found to be the same; in farther proof of which it may be added that they were met with, as well as those in fig. 18, all lying together§. The membrane, *h*, is shown completely formed in nearly all the other figures of the same Plate. This is the membrane which I have already described in Mammalia, and intend describing in other Vertebrata, as the *ovisac*. See the second column in the Table, page 304.

The order of Formation of the several parts of the Ovarian Ovum.

24. After the formation of the ovisac, the germinal vesicle, as already said (12.), is generally for a short time concealed. This is perhaps partly referrible to minute oil-like globules being mixed with the peculiar granules of the ovisac, and causing great refraction. Liquefaction, however, of some of the granules appears to take place, or a fluid from some other source is added, and then the germinal vesicle is seen in or

† = $\frac{1}{111}$ of an English inch. ‡ And probably in Mammalia generally.

§ All of these objects, moreover, were lying among newly-formed ovisacs.

near the centre of the ovisac. The germinal vesicle is here surrounded by the peculiar granules of the ovisac, either immediately† (Plate V. fig. 10. *c.*), or having minute oil-like globules interposed‡ (fig. 14. and 15. (*)). The latter, with a pellucid fluid, indicate the incipient formation of the yelk around the germinal vesicle§. The oil-like globules accumulate (fig. 15. (*)), and minute opake granules gradually present themselves among them. When the formation of the yelk has thus proceeded to a certain stage, two membranes are seen surrounding it; one of which is the proper membrane of the yelk, *membrana vitelli* (49.), and the other, more external, is the true *chorion*‖ (52.) (fig. 17. *e.* and *f.*). The ovum is seen with great distinctness through the transparent membrane of the ovisac, and it is thus possible to follow its several stages of formation. Subsequently, a covering or tunic, consisting of a kind of dense cellular tissue susceptible of becoming highly vascular, and closely connected with the surrounding stroma, is gradually formed upon the outer surface of the ovisac, which previously in a high degree transparent, now becomes translucent only.

The Folliculus Graafianus, or Graafian Vesicle.

25. From the union of the ovisac with the covering or tunic now mentioned, I apprehend it is that there results what has been called a *Graafian vesicle*; and it appears to me that the "Couche *interne* de la capsule de la vésicule de De Graaf" of Baer (Plate VII. fig. 62. *h.*) is in reality the previously independent *ovisac*, while the "couche *externe*"¶ of this author (Plate VII. fig. 62. *i.*) is the *covering* or *tunic* of the ovisac above mentioned.

26. Professor Baer has expressed his belief that the formation of the "ovulum" (ovum) precedes that of the Graafian vesicle; but he adds, "Je doute qu'il soit jamais possible à l'homme de s'en convaincre par l'observation††." I trust that the facts mentioned in the preceding pages, and illustrated in Plate V., will suffice to show how far this eminent naturalist was right in his conjecture as to the priority in formation of the ovum; and that they will also serve to demonstrate the possibility (questioned by Baer) of determining this point *by observation*.

† See the third column in the Table, page 304. ‡ See the fourth column in the Table, page 304.

§ The germinal vesicle is thus in or near the centre of the yelk when the latter is first formed. This vesicle subsequently passes to the surface of the yelk.

T. W. Jones (London and Edinburgh Philosophical Magazine, No. 39, Sept. 1835, p. 209.) mentions the germinal vesicle as "having on one side a small elevation, which, projecting among the grains composing the granular sac, fixes the vesicle in its place." I have never observed this "small elevation," nor do I find any mention to have been made of it by either Coste or Valentin, who had previously described the germinal vesicle in Mammalia, or by those who have since written on it.

‖ See the fifth column in the Table, page 304.

¶ Von Baer says of this "couche externe," "Elle reçoit des vaisseaux, les contient dans son intérieur, et leurs extrémités vont dans la couche suivante" ("couche interne"). Lettre, &c., p. 17.

†† Lettre, &c., pp. 20, 21.

The Ovisac of Birds ("Chorion" of Authors) compared with the Ovisac of Mammals.

27. The ovarian calyx of the Bird, if deprived of its peritoneal investment, and what there is of the parenchyma† of the ovary, would, I apprehend, present *a structure analogous to the Graafian vesicle of Mammals*, that is, *a structure consisting of an ovisac that has acquired a proper* (*and in the later stages a highly vascular*) *covering*. Yet the ovisac of Birds has been mistaken for a vesicle corresponding to the *chorion* of Mammals, and accordingly it has been called the "chorion." The impropriety of this designation will appear evident if the vesicle in question be followed through its several stages of formation, which I do not find to have been hitherto attempted. For this purpose I need not do more than refer to the several figures.

28. Plate V. fig. 22. Ovisacs of the Pigeon, $\frac{1}{25}'''$ to $\frac{1}{12}'''$. Germinal vesicle (*c*) in the centre, surrounded by oil-like globules (*), the incipient yelk. Compare with fig. 15. from the Hog.

29. Plate V. fig. 23. Ovisac of the Common Fowl, $\frac{1}{60}'''$, with similar contents. Compare with fig. 15. from the Hog.

30. Plate V. fig. 24. Ovisac of the common Fowl, $\frac{1}{77}'''$. Yelk (*d*) just formed, but the membrana vitelli not yet visible.

31. Plate V. fig. 25. Ovisac of the common Fowl, $\frac{1}{14}'''$. Membrana vitelli (*e*) now seen. Compare with fig. 16. from the Hog.

32. Plate V. fig. 26. Ovisac of the common Fowl in a stage much more advanced; *g*, peculiar granules (8.). The yelk (*d*) has become in some degree opake. The ovisac has now a proper covering or tunic, and presents by this addition a structure (capsule) (*h i*) analogous to the Graafian vesicle of Mammals. The quantity of yelk in Birds is very large; hence that portion of the ovary which contains the structure now described (as analogous to the Graafian vesicle of Mammals) becomes pendent, and now the united coverings of the yelk-ball, viz. the ovisac, its proper tunic, the parenchyma of the ovary, and the peritoneal investment, are together called the *calyx*, which is connected with the ovary by a pedicle‡.

33. Plate VIII. fig. 76. Ovisac of the Pigeon, with its vascular covering or tunic. The yelk has escaped, and large granules, essentially the same as those in the ovisac of Mammalia (8.), are seen slowly passing out.

† This term is here intended to include the "*capsula propria*" of authors, for the same reason as that which influenced Baer in declining to admit any distinction, except a difference in density, between his *stroma* and the so-called "*tunica albuginea*" in the ovary of Mammals.

‡ When a minute incipient capsule is crushed under the microscope, the yelk is observed to escape into the pedicle. If the ideal section (Plate VI. fig. 38½.) be referred to, it will be seen that the yelk, in taking this direction, ruptures the parietes of its containing cavity at a point where it meets with the least resistance. It ruptures, not the whole thickness of the capsule, but its internal membrane only, formerly the ovisac. (In this instance (Plate V. fig. 26.) the yelk is seen to have subsequently ruptured also the pedicle itself, and appears escaping through the opening.)

The Ovisac of Amphibia and Fishes ("Chorion" of Authors) compared with the Ovisac of Mammalia.

34. The ovisac of Amphibia, and that of Fishes, improperly called the "chorion," first presents itself in a very similar condition to the ovisacs of Mammalia and Birds, viz. *size*, the 50th of a Paris line † and less; *form*, generally elliptical or ellipsoidal; *contour*, in plaits or folds. The chief difference consists in the fluid of the ovisac not generally (though in some instances) presenting the peculiar granules (8.) described as occurring in Mammalia and Birds.

35. Plate VI. fig. 31. Ovisacs of the Cod, $\frac{1}{40}'''$ to $\frac{1}{10}'''$. Contour in folds. Germinal vesicle (*c*) in the centre, with its numerous germinal spots (*b*) ‡.

36. Plate VI. fig. 27. Ovisac of the common Frog, ("chorion" of R. Wagner §, from whom I take this figure,) $\frac{1}{10}'''$: *c*. germinal vesicle; *b*. germinal spots (many ‡); *e*. membrana vitelli, yelk pellucid.

37. Plate VI. fig. 32. Ovisacs of the Whiting, $\frac{1}{7}'''$ to $\frac{1}{4}'''$. There is a trace of yelk (*d*), with something like the peculiar granules (*g*) of the ovisac in Mammalia and Birds in the fluid of the ovisac. Compare with fig. 10. Plate V. from the Cat, and fig. 21. Plate V. from the Pigeon.

38. Plate VI. fig. 33. Ovisacs of the Haddock. Yelk (*d*) circumscribed by its proper, and here very delicate membrane.

39. Plate VI. fig. 27½. Ovisac ‖ of the *Coluber natrix*: *b*. germinal spot (single ‡); *c*. germinal vesicle; *d*. yelk; *e*. membrana vitelli.

40. Plate VI. fig. 28. Ovisacs of the common Frog, $\frac{1}{3}'''$: *b*. germinal spot; *c*. germinal vesicle; *e*. membrana vitelli; *h*. proper membrane of the ovisac; *d'*. a spot which I always find on the internal surface of the membrana vitelli of the Frog in ovisacs of about this size. This spot does not appear to have been hitherto described. It is generally elliptic, rarely round, has a well-defined contour, and is perhaps slightly lenticular in form. In this instance it measured $\frac{1}{15}'''$ in length, and is often of about the same size. It appears to be composed of granules.

41. Plate VI. fig. 34. Ovisac of the Turbot, $\frac{1}{10}'''$: *e*, membrana vitelli now visible; *d*. yelk, with oil-like globules (*) around the germinal vesicle. Compare with fig. 25. Plate V. from the common Fowl, and fig. 16. Plate V. from the Hog.

42. Plate VI. fig. 35. Ovisacs of the Salmon, $\frac{1}{10}'''$ to $\frac{1}{4}'''$, in nearly the same stage of formation as the ovisac of the Turbot, fig. 34. At *g* are seen some of the peculiar

† = $\frac{1}{87}$ of an English inch.

‡ Professor R. Wagner finds a *single* spot in cartilaginous, and *many* spots in osseous Fishes. He finds also a *single* spot in the scaled, and *many* spots in the naked Amphibia.

§ Beiträge zur Geschichte der Zeugung und Entwickelung, tab. ii. fig. 6. *a*. (aus den Abhandlungen der Mathematisch-physikalischen Klasse der Königl. Bayer. Akad. der Wissenschaften in München besonders abgedruckt).

‖ "Chorion" of R. Wagner, from whom I borrow this figure. (Beiträge, &c., tab. ii. fig. 7 *c*.)

granules (8.) in the fluid of the ovisac. Compare with fig. 16. Plate V. from the Hog, and fig. 25. Plate V. from the common Fowl.

43. Plate VI. fig. 29. Ovisac of the common Frog; the yelk (*d*) becoming opake, and the germinal vesicle (*c*) in its centre.

44. Plate VI. fig. 36. Ovisac of the Salmon, acquiring a proper covering or tunic. In this instance two minute ovisacs are *included* (57.) by that portion of the covering which enters into the formation of the pedicle.

45. Plate VI. fig. 37. Ovisacs of the Salmon, no longer to be distinguished from their proper coverings or tunics, with which they have coalesced to form vesicles (capsules) analogous to the Graafian vesicle of Mammals. The vesicles have been crushed, and the yelk is seen escaping by the pedicles, taking with it its proper membrane (*e*). See the ideal section, Plate VI. fig. 38½; which shows why the yelk escapes by this passage.

46. Plate VI. fig. 38. Perfect calyx of the Salmon, with its pedicle.

The Graafian Vesicle of Mammals, how related to the Calyx of Birds, Amphibia, and Fishes.

47. Should the accuracy of the figures now compared be confirmed by future observation, it may perhaps be considered as established, in the first place, that the primitive elements of the ovum of Birds, Amphibia, and Fishes, are contained in a vesicle (the "chorion" of authors) essentially the same as that which I have called the *ovisac* of the Mammalia; secondly, that if it be, as I suppose, by acquiring a proper covering or tunic† that the ovisac of the Mammalia becomes what has been by others called a *Graafian vesicle*, the ovisac of Birds, &c. by acquiring a proper covering or tunic† presents a structure (capsule) analogous thereto; and thirdly, that it is the structure so constituted in the Bird, &c., which on becoming pendent from the ovary, and invested by what there is of the substance of the ovary, as well as in some instances by the peritoneum‡, has been called a calyx§.

48. From the foregoing it appears that the Graafian vesicle is not, as it has been supposed to be, a structure peculiar to Mammalia. Nor is it correct to consider the Graafian vesicle of Mammalia as analogous to the whole calyx of other animals. It corresponds to no more than the two internal membranes, viz. the ovisac and its proper vascular covering, the remainder of the calyx being derived from the ovary, with, in Birds, some Amphibia and some Fishes, a peritoneal covering; and however incon-

† Susceptible, as already said, of becoming highly vascular.

‡ In other instances the peritoneal investment does not enter into the formation of the calyx. We find examples in the naked Amphibia, in Ophidian and Saurian Reptiles, and in most osseous Fishes, where the ovaries are sacs, having the calyces pendent in their interior.

§ My researches have not yet been extended to the Invertebrata; but I am by no means disposed to consider the existence of the vesicle, which I have called the ovisac, as limited to vertebrated animals.

siderable this portion of the calyx may appear, yet for the sake of analogy the distinction is not unworthy of being made†.

The proper Membrane of the Yelk in Mammals.

49. Several distinguished authors have not described this membrane‡. And in fact it is not easy to demonstrate the separate existence of such a membrane in the class Mammalia generally, at those periods in the formation of the ovum when the latter is usually examined. On the other hand, a very accurate observer, Krause, has figured it in the Goat§, and appears to have often seen it in other Mammals. My friend Professor Rudolph Wagner pointed it out to me in the Cat; and he seems convinced from analogy that its existence must be general in the class Mammalia, but does not appear to have discerned it in any other instance‖. I have since met with and figured it in early stages of the ovum of the same animal (the Cat), and I believe also in the Hog (Plate V. figs. 17. and 16. *e.*; Plate VIII. fig. 70. *e.*). In later stages, *ante coitum*, I have sought this membrane in vain as a distinct structure¶; but I think it not improbable that the well-marked line generally regarded as representing the internal surface of the thick chorion, may be partly due to the proper membrane of the yelk, as is obviously the case in Plate V. fig. 17. *e.* and *f.*

50. It appears to me that the existence of a proper membrane of the yelk in the ovary throughout the class Mammalia, may be inferred from what we know of the effect observed to be produced on ova by maceration; when the thick chorion, imbibing fluid into its interior, becomes distended beyond the size of the yelk-ball, but the latter *retains its form*, which certainly would not be the case were not the yelk circumscribed by a proper membrane (Plate VIII. fig. 72. *f.* and *d.*††).

51. I have observed a very interesting fact connected with the proper membrane of the yelk *post coitum* not hitherto made known, which not only adds probability to the supposition that this membrane has a previous existence, but also accords in some degree with a change known to take place in Birds. I find that in the Rabbit,

† Professor R. Wagner in his examination of ova of the Frog, finding his "chorion" (ovisac) to disappear, makes the following inquiry: "Ist sie nun mit der Dotterhaut verschmolzen oder hat sie, was wahrscheinlicher ist, sich an die gefässreiche Kelch-membran angelegt und bildet sie nur die innere Lamelle derselben?" (Beiträge, &c., S. 76.) This inquiry, made by one of the most accurate of observers, bespeaks, I think, the perception, in a single instance, of something that was very near the truth.

‡ Among these are Coste (Embryogénie Comparée, p. 80.) and T. W. Jones (London and Edinburgh Phil. Mag., No. 39, Sept. 1835, p. 210.).

§ Müller's Archiv, 1837, Heft I. Taf. I. figs. 4, 5, 6.

‖ Beiträge, &c., S. 20.

¶ Since the above was written, an examination of the ovum of the Goat enables me to attest the accuracy of Professor Krause in his representation of the membrana vitelli in this animal, in which it is exceedingly distinct. I did not however, in any instance find the membrana vitelli surrounded by a fluid as described by Krause (53. Note), but by the perfectly formed and consistent chorion.

†† I borrow this figure from the excellent Thesis of Bernhardt (Symbolæ, &c., fig. xxiii. p. 45.), but am compelled to offer an explanation of several of its parts different from that given by the author. This is deferred, however, until after the description of the chorion (56. Note). See also the Description of the Plates.

just before the ovum leaves the ovary, this membrane, previously so delicately thin, becomes perfectly distinct and very thick; and that the chorion, imbibing fluid into its interior, becomes somewhat distended, so that a minute space is visible between the membrana vitelli and the chorion. This thickening of the proper membrane of the yelk, and the distention of the chorion, subsequently proceed much farther, as is proved by the state of ova found in the Fallopian tube. I find also, that the membrana vitelli is still visible, and has considerable thickness in minute ova met with in the uterus. This subject will be entered into more fully in a future paper.

The true Chorion, a Structure superadded within the Ovary in the Class Mammalia.

52. The figures above referred to, in comparing the *ovisac* of Mammalia with that of Birds, Amphibia, and Fishes, present also up to a certain period a perfect analogy between the rudiments of the *ovum* itself in these four classes. We have seen in all, the germinal vesicle and its contents, as well as the yelk, and proper membrane of the yelk. Here, however, the analogy is terminated by the formation, within the ovary, in Mammalia, of a membrane to which there is no corresponding structure *within the ovary* in other Vertebrata. This membrane appears to be the true *chorion*. In the ovary of Birds, Amphibia, and Fishes, it is, I believe, allowed that there is no membrane formed external to the membrana vitelli†. The body therefore expelled from the ovary in these animals is not an ovum, but a *yelk-ball*. The subject will be illustrated by the following Table, showing the parts present (in a mature state) in the ovary of Mammalia on the one hand, and of Birds, &c. on the other:—

Mammalia.	Birds, some Amphibia‡, and some Fishes‡.
Germinal vesicle (*c*) and its contents.	Germinal vesicle (*c*) and its contents.
Yelk, *d*.	Yelk, *d*.
Membrana vitelli, *e*.	Membrana vitelli, *e*.
Chorion, *f*.	
§ *Tunica granulosa*, g^1.	
§ *Retinacula*, g^2.	
Fluid, granules (*g*), and oil-like globules (*).	
§ Membrana granulosa, g^3.	
Ovisac, *h*. *Vascular tunic*, *i*. } = Graafian vesicle, *h i*.	*Ovisac*, *h*, *Vascular tunic*, *i*. } = Capsule, *h i*
Stroma, *k*.	Stroma, *k*.
Peritoneal covering, *l*.	Peritoneal covering, *l*. } = Calyx, *h i k l*.

† Professor Rathke, however, finds that in certain Fishes, not provided with an oviduct, the "schaalenhaut" is a production of the ovary. (Burdach's Physiologie, 1837, Band II. § 339.)

‡ In other Amphibia and most of the osseous Fishes, the peritoneum does not enter into the formation of the calyx (47. Note).

§ To be described in Part II. of this Memoir (64. 80. 72.).

The foregoing Table shows that in the ovary of Mammalia, the chorion is superadded; and as I shall demonstrate in a future communication (what others, and particularly Coste and R. Wagner, have conjectured) that this membrane is really the chorion of ova met with in the uterus†, it may be said that not merely a yelk-ball, but an *ovum* is expelled from the ovary of Mammals‡.

53. The chorion, as it exists in the ovary, is soft, very thick, and remarkably transparent. It closely invests the membrana vitelli, so as in nearly all instances to conceal the separate existence of this very delicate membrane (49.). When first formed, the inner portion of the substance of the chorion is probably in a fluid state, as is evident from Plate VIII. fig. 70. *f*., in which instance slight pressure at one side forced a part of the substance of this membrane to the side opposite§. (In this figure the inner circle (*e*) is probably the proper membrane of the yelk (49.).) Subsequently the chorion acquires more consistence; for if it be ruptured no collapse takes place even after the escape of its contents, and the membrane continues throughout of equal and undiminished thickness‖. I find, as already said (51.), that just before the ovum leaves the ovary this membrane begins to be attenuated by imbibition of fluid into its interior, and consequent distention. This imbibition of fluid and distention may be produced by maceration (Plate VIII. fig. 72.).

54. The great thickness and transparency of the chorion, as it exists in the ovary, long prevented its real nature from being ascertained, and appear to have been the means of misleading several excellent observers.

55. Von Baer, for instance, after describing the yelk as "une sphère creuse à paroi épaisse," says it is separated "par un *intervalle transparent* d'une autre sphère creuse à paroi mince¶." The "intervalle transparent" of Professor Baer is obviously the transparent substance of the chorion, and his "paroi mince" the external surface of this thick membrane. It appears to have been this supposed "paroi mince" that Baer considered as subsequently forming the "membrane corticale" of ova in the uterus.

† The membrane here denominated the true chorion is considered by Coste as the "membrane vitelline" (Embryogénie Comparée, p. 80.). This author, however, as above said, is correct in supposing this membrane to become the chorion in the uterus.

‡ On the other hand, there is a structure formed in the ovary of Birds, and some other Vertebrata, which (though it has been described) I do not find existing *ante coitum, in the ovary* of Mammals. This is the incipient "germinal membrane" ("blastoderma"), to be considered in a future paper.

§ Professor Krause supposes the membrana vitelli to be surrounded by a pellucid fluid, which is circumscribed by an extremely delicate membrane (Muller's Archiv, 1837, Heft I. S. 27, 28; see also the description of these parts in Krause's admirable work, "Handbuch der menschlichen Anatomie, Band i. S. 557, 558."). The Professor's description I find very accurate, as applied to the chorion in an early state; such, for instance, as that which I have figured in Plate VIII. fig. 70, above referred to; or possibly it may be applicable to the permanent condition of this membrane in certain animals. I have never met with this state, however, except in ova newly formed.

‖ Bernhardt has thus demonstrated the nature of this membrane (Symbolæ, &c. fig. xx.); and a figure by R. Wagner (Beiträge, &c., Tab. i. fig. 2.) shows it in a manner still more satisfactory.

¶ Commentaire, pp. 39, 40.

56. Bernhardt, as already said, by rupturing this membrane demonstrated its real nature. In cases, too, where he viewed the "ovulum" (ovum) freed from its surrounding granules, he appears to have regarded it, in its whole thickness, as the "membrana externa ovuli†;" but in those instances in which granules were adherent to the ovum, concealing the outer surface of this thick membrane, the substance of the latter was mistaken by Bernhardt for a "zona pellucida," or "spatium pellucidum;" and the part which this author indicates as the "membrana ovuli externa," is obviously the internal surface of the chorion. See Plate VIII. fig. 72.‡, and the description of the Plates.

Parasitic Ovisacs.

57. Minute ovisacs (*h*) are seen in Plate VIII. fig. 75, in the parietes of a Graafian vesicle (*h i*) of one of the Mammalia, the Dog. The same Plate, fig. 76, presents a number of ovisacs similarly situated in the corresponding structure of a bird, the Pigeon §. If such a Graafian vesicle or corresponding structure be crushed, the ovum contained in its cavity escapes, but the minute ovisacs in its parietes remain unmoved. I have counted more than fifty of the latter, scarcely exceeding their primitive size, in the parietes of one of these structures from the Bird, none of which probably would have been matured. Sometimes these minute ovisacs appear to have *originated* in the substance of the covering acquired by a larger ovisac; and sometimes they seem to be *included* by the covering which the larger ovisac acquires. An instance of the latter is seen in Plate VI. fig. 36, where two small ovisacs are so included‖. The minute ovisacs in such a situation, whether originating there or contained in it by inclusion, I propose to call *parasitic ovisacs*.

58. Plate VIII. fig. 77. presents one of these parasitic ovisacs¶ (*h*) contained in the parietes of a Graafian vesicle of the Ox. Both have been divided, one half of each remaining in the ovary, the other being reflected. At fig. 78. is an enlarged view of the two halves of the divided parasitic ovisac¶ seen in fig. 77. The proper membrane (*h*) is seen lined by the membrana granulosa (g^3); and within the latter is a dark space, the cavity of the parasitic ovisac which contains the ovum (*f*). Fig. 79. presents a transverse section of this parasitic ovisac; *h* being its proper membrane, and *g* granules of the membrana granulosa. The proper membrane (*h*) of the parasitic ovisac in this instance measured $\frac{1}{4}^{m}$ in thickness, and its cavity distended under the compressor $1\frac{1}{2}^{m}$ in length.

† Symbolæ, &c., figs. ii. iii. iv. x. &c.

‡ Fig. xxiii. of Bernhardt. This is the figure before referred to in the description of the proper membrane of the yelk (50. Note).

§ I have seen minute ovisacs similarly situated in a Chelonian reptile.

‖ In this instance the included ovisacs are contained in the incipient *pedicle* already mentioned (44).

¶ Incipient Graafian vesicle?

Isolated Spots in the Graafian Vesicle.

59. These *appear* to be of two kinds, but are really the same,—in the one case seen single, in the other grouped. Plate VIII. fig. 66. g^*. presents the appearance of these objects in the one state, and Plate V. fig. 26½. their appearance in the other. Each of these spots I find to consist of one of the peculiar granules of the Graafian vesicle, having a peripheral accumulation consisting of oil-like globules†. The peculiar granules are transparent and comparatively large; the oil-like globules appear dark in their circumference by reason of a very different refracting power from that of the suspending fluid; they are also of a minuteness quite immeasurable. When these objects are in small number, they present the appearance shown in Plate VIII. fig. 66. g^*.; and when in very large number, that presented in Plate V. fig. 26½.‡.

Disappearance of Ova, and Formation of others.

60. I have very often met with ova in the state represented in Plate VIII. fig. 67, which was observed in a Rabbit of about ten weeks old. The chorion (*f*) has become thin, distended, and of an elliptic form. It measured in this instance one-fifth of a Paris line in length§. The yelk (*d*) is nearly black, consisting chiefly of granules of extreme minuteness, with some oil-like globules and a fluid. Its membrane, if still existing, is not distinguishable from the chorion; and indeed the chorion itself is scarcely to be distinguished from the yelk. The germinal vesicle (*c*) is generally situated at one end of the ovum, as in this instance,—appears more or less flattened or collapsed,—and is much enlarged. It measured in this case $\frac{1}{1}\frac{1}{5}$th of a line in length. The germinal spot (*b*) also was enlarged, having measured in this instance $\frac{1}{1}\frac{1}{5}$th of a line in length, for it was somewhat elliptical. Its contour is not well marked. The colour of the Graafian vesicle, with such a condition of its ovum, is often tinged with yellow. In young Rabbits this state of the ovum has presented itself so frequently, that I have observed several in the same field of view. I apprehend it to denote a stage in the absorption of ova; which on the other hand appear to be continually replaced by new formations‖.

† Among the oil-like globules there are sometimes visible a few minute opake granules.

‡ Spots which I suppose to represent the first of the two states now described, have been figured in Bernhardt's Dissertation, Symbolæ, &c., fig. xvii.; but the author's description of them does not quite accord with that which I have given.

§ = $\frac{1}{1}\frac{1}{2}$ of an English inch.

‖ In a future paper it will be shown that, *post coitum*, many ova are absorbed. In Plate VI. figs. 44. and 46. are seen two ova in which absorption is incipient, as indicated by a loosening of the granules, and consequent enlargement of the tunica granulosa (g^1),—by the more globular form of some of these granules (9.),—and by liquefaction of the yelk around the germinal vesicle.

PART II.

On a granulous Tunic of the Ovum of Mammalia not hitherto described; on the manner of origin of the "Membrana Granulosa" of Authors; on the different situations of the Ovum in the Graafian Vesicle at certain periods ante coitum, not hitherto observed; and on certain structures by means of which the Ovum is made to occupy those several situations.

61. Professor Baer has described and figured the "ovulum" (ovum) of Mammalia as situated in a mass of granules at or near to the periphery of the vesicle of Graaf (Plate VII. fig. 62.), and the following is the substance of the description he has given.

The "Disc" of Professor Baer.

62. This accumulation of granules, according to Baer, is not globular but discoid†, thicker at the central part‡, and surrounding the "ovulum" like the frame of a lens its glass, but probably extended so as completely to inclose the under surface of the "ovulum"§ in addition to its sides. The lateral portion of this mass of granules Von Baer calls the disc ("*discus proligerus*")‖, and the central thickened part he calls the "*cumulus strati proligeri*‖." This description is intended to apply to this part in the Mammalia generally; but Von Baer describes the form of the "cumulus" as varying in different animals, and in the same animal at different periods, perhaps according to its degree of maturity. Thus it is very inconsiderable in the Dog, requiring the greatest attention to discern that the disc is thickened at its middle: and here Von Baer compares the granulous accumulation to a plate used at table, as seen on its lower surface. In the Cow he has seen the "cumulus" cylindrical or globular. This convex middle part is also very considerable in other animals; in the human female it has the appearance of a depressed hemisphere; and in the Hog it presented very varied forms¶, the cause of which differences Baer was not able to explain††. In the Cow he saw a plane granulous substance, very distinct from the eminence ("cumulus"), extending itself at the surface of the fluid of the Graafian vesicle; but whether this was situated internal to the cumulus, or formed a part of it destined to be detached, he considered extremely difficult to decide‡‡. He adds, "when we examine an ovulum of the Sow or Cow under the microscope, we there see commonly adherent a portion more or less considerable of a flat disc. In the Sow I have seen this little membrane bounded by a circular border; I believe that I have sometimes obtained it entire. In the Dog the disc is always limited by a circular border, but in this animal the membrana granulosa is too small to admit of being well examined. In the ovary of the human female I think I have distinctly

† Lettre, &c., p. 15. ‡ *L. c.*, p. 16. § *L. c.*, p. 16.

‖ He gives it this name from the supposition of its analogy with the granules surrounding the germinal vesicle of Birds.

¶ *L. c.*, p. 16. †† Commentaire, &c., p. 40. ‡‡ Commentaire, &c., p. 40.

seen the membrana granulosa to pass above the discus proligerus. On the immature ovulum [ovum] of the Cow, on the contrary, I have recognised no more than a continuous lamina, which I consider as the membrana granulosa. From all that has been said, I think I may conclude that the ovula [ova] of the Mammalia, those at least which are approaching maturity, are plunged into a couch, formed of a thick eminence, the cumulus, and of a flat disc, and that the latter may or may not be primitively isolated from the membrana granulosa; development predominating sometimes on the part of the disc, sometimes on the part of the cumulus, examples of which are offered (respectively) by the Bitch and Cow†." See Plate VII. fig. 62. and Plate VIII. fig. 80.‡.

63. This description appears to be accurate in a few particulars, but for the most part it does not accord with my observations. I have naturally felt some hesitation in stating this, not only from its being opposed to the opinion of so experienced and, generally speaking, so accurate an observer, but because those who have succeeded Baer in these researches appear to have adopted entirely his description of this structure. In now offering a different account of it, I would express my belief that should this prove more accurate, it will have been rather due to my examination of the ovum when situated in the *centre* (as well as near the surface) of the Graafian vesicle, than to imperfect observation on the part of those eminent observers. But I must first describe a structure which, to my surprise, has not been hitherto observed, though visible in those situations in which the ovum has been usually examined.

The Tunica granulosa.

64. On the rupture of a Graafian vesicle under the compressor, the ovum escapes, and with it a mass of the peculiar granules I have at some length described (8.). This mass of granules, as hitherto obtained, has presented the appearance of a lacerated structure, and been regarded simply as a portion of the "disc" of Baer§. I find, however, that if the mass thus escaping be attentively examined, those granules that immediately surround the ovum appear to be in a state of denser aggregation than the rest, from which they are to be distinguished therefore by a circumscribing line. If the granules thus circumscribed be viewed at different distances, they are seen to invest the whole surface of the ovum, forming a tunic, which is perfectly spherical in form (Plate VI. figs. 42. to 47. g^1.). I have frequently obtained the ovum invested by no other granules than those of this tunic; that is, this tunic has been freed from the other granules, which on the bursting of a Graafian vesicle generally escape adherent to it; the fact being, that all that properly belongs to the ovum of the mass of granules in which it is found imbedded in the Graafian vesicle is this

† Commentaire, &c., p. 40.

‡ Lettre, &c., figs. ix. and xii. of Baer. These figures I have introduced for the purpose of contrasting them with my own drawings.

§ This lacerated structure is the "zona granulosa" of Bernhardt, Symbolæ, &c., figs. i. vii. xviii. xxi. xxiii. See also (in the present memoir) Plate VIII. fig. 72. g^1., which is taken from that author.

tunic, between which and the surrounding mass there is much less adhesion than there is between the granules of either among themselves. Having never failed to find this tunic enveloping mature ova in the Mammalia, I believe its presence to be constant and essential; and I am also of opinion that in form, substance, and situation it is essentially the same throughout this class of animals. I propose to call it the granulous tunic of the ovum, *tunica granulosa*.

65. At a certain period this tunic, in some animals at least, is seen to have tail-like appendages consisting of granules precisely similar to its own (Plate VII. fig. 63. g^1.). These appendages appear to be frequently four in number, and their direction corresponds to that of other granulous cords or bands to be presently described (86.). They are not very distinctly seen while within the Graafian vesicle, and in this situation I have not figured them.

66. Should the existence of this structure as a *tunic* be confirmed by the observations of others, it will no longer be surprising that the outer line in the double contour of the thick chorion remained so long unseen, and that this thick membrane was regarded as a "zone," "halo," or "pellucid space." For the same reason it will be obvious why it has been found so difficult to free the ovum from the surrounding mass of granules; though I find that these granules do not adhere to the chorion they invest, so closely as to each other.

67. The tunica granulosa may be obtained in an uninjured state, either by carefully opening a Graafian vesicle of considerable size, and receiving its fluid and granulous contents into a watch-glass (Plate VII. fig. 61. g^1.), or by bursting one of middling size under the microscope with the compressor, when the escape of the ovum in its tunica granulosa may be observed. In both cases, however, portions of another granulous structure (the retinacula, to be presently described (80.),) generally escape adherent to it. This tunic is also very distinctly visible *in situ*, as in Plate VI. figs. 39, 40. and Plate VII. figs. 55. to 59. g^1.

68. I have met with the ovum when immature, and apparently just after the formation of the chorion (Plate V. fig. 17. *f*.), at which period the tunica granulosa does not exist; and in subsequent stages its gradual formation may be observed, as in Plate VI. fig. 41. g^1., where its thickness is inconsiderable and its contour irregular.

69. The tunica granulosa, as already said, is spherical. In whatever direction it is viewed, whether as in Plate VII. fig. 61. g^1., or in any other of the figures of this Plate, there is perceptible no difference in its form; the distinctness with which the contained ovum is seen, depending, not on the absence of the investing granules at any part of its periphery, but on the extreme transparence of these granules. The flattening at one side which this tunic may undergo, and the entire deprivation of granules, perhaps, at one point of the surface of the chorion, by pressure against the membrane of the ovisac, I shall have to notice (88.)†.

† I shall presently describe a structure supposed, as already said (56.), by Professor Baer to be "discoid" in its form. In the *Dog*, however, this author has obviously figured as the "*discus proligerus*" the structure

70. The granules forming this tunic being soluble in water (9.), the fluid of a large Graafian vesicle, or some thin albumen, is a preferable medium in which to view this structure, as well as all the other parts connected with the ovum.

71. In a future paper I shall show that the tunica granulosa accompanies the ovum into the Fallopian tube.

The Membrana granulosa.

72. Von Baer† gave this name to a stratum of granules lining the internal surface of the Graafian vesicle. That author's description of the membrana granulosa appears to be very accurate so far as it goes; but there remains something to be said respecting it, for to this day doubts are entertained whether this stratum of granules really constitutes a membrane, though Dr. Pockels recently demonstrated it to be such in a mature Graafian vesicle of the Deer‡. Von Baer has also figured it§, but so little magnified that no adequate idea is thus given of this membrane.

73. It is composed of the peculiar granules of the ovisac (Plate VIII. fig. 71.). With the proper membrane of the ovisac, now the lining membrane of the Graafian vesicle, the membrana granulosa is in contact only‖, appearing very easily to break down and separate from it. The membrana granulosa may be made to escape by rupturing a Graafian vesicle under the compressor; when, after the escape of the other contents, it slowly passes out in a membranous form¶. It is then possible to roll portions of this membrane on themselves, and even to obtain folds, as seen in Plate VIII. fig. 71.

74. The granules forming the internal surface of this membrane are sometimes found less densely aggregated together than those in other parts; portions of the membrane projecting like villi into the fluid of the Graafian vesicle. This membrane is also frequently found to be of very unequal thickness, projecting at some parts much farther into the cavity of the Graafian vesicle than at others. The cause of these apparent villosities and projections will be presently explained, when the manner of origin also of this membrane will be shown (79.).

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which I am now describing as the *spherical* tunica granulosa. He probably saw it in no other state than that to which I have just referred, viz. flattened or removed by pressure against the membrane of the ovisac.

† Lettre, &c., p. 18, and Commentaire, p. 47.

‡ Müller's Archiv, 1836, Heft ii. S. 193—204. Tab. vi.

§ Lettre, &c., fig. xi.

‖ It was observed by Baer, that between the membrana granulosa and the internal surface of the Graafian vesicle there was no "liaison organique." Commentaire, p. 41.

By maceration, as observed by Baer also, the membranous structure of this granulous lining may be well seen *in situ* from the exterior of a Graafian vesicle; the membrana granulosa separating as a distinct vesicle, and a space intervening between it and the walls of the cavity it lines.

¶ It is often more yellow in its colour than the other granulous contents of the Graafian vesicle.

The Ovum in the centre of the Ovisac. Manner of origin of the Membrana granulosa.

75. The formation of the ovum, shown in Part I. of this memoir (12—22.) to commence before the existence of the ovisac, is completed in the number of its parts, (though probably not matured) in or near the *centre* of this vesicle (Plate V. fig. 17.).

76. But if the ovum at this period is situated in the centre of the ovisac, by what means is it supported there? At first this appears to be effected by an equable diffusion of the peculiar granules of the ovisac throughout the fluid of this vesicle (Plate V. fig. 17.). Subsequently a peculiar structure arises partly for this purpose.

77. The ovisac, as already said (24. 25.), acquires a proper covering or tunic susceptible of becoming highly vascular, and thus passes into the condition of what has been denominated a Graafian vesicle. As this covering or tunic begins to form at the period just referred to, viz. while the ovum is still in the *centre* of its containing cavity, it will be proper in future to lay aside the term *ovisac* and to substitute that of *Graafian vesicle*, the former being now no more than the lining membrane of the latter.

78. The granulous contents of the *Graafian vesicle* now assume an amygdaloidal appearance (Plate VII. fig. 48.), which seems to arise from a denser aggregation of the granules in certain parts, and the occupation, by the fluid, of the spaces thus occasioned; in other words from a *histological* separation. If examined more closely, however, the condition of the interior of the ovisac is not found to be really amygdaloidal, as the cavities communicate with one another (Plate VII. fig. 49.).

79. In a stage somewhat more advanced this separation is found to have proceeded farther, the peculiar granules of the Graafian vesicle having arranged themselves so as to constitute three distinct structures (Plate VII. fig. 50. g^1, g^2, g^3.). Some of them are collected on the surface of the chorion, forming the *tunica granulosa* (g^1), which I have already described (64.); others constitute a structure, or rather an assemblage of structures, consisting of a central mass, in which the ovum (in its granulous tunic) is contained, and of cords extending from the membrana granulosa to this central mass (g^2), which will be now described; and the third portion of these granules is collected on the inner surface of the Graafian vesicle, where they constitute the membrana granulosa (g^3), *this being, I apprehend, the manner of origin of this membrane.* See the sixth column in the table, page 304.

The Retinacula.

80. The central mass and cords of granules just mentioned (Plate VII. fig. 50. g^2.) do not appear to have been hitherto observed. One of their uses will perhaps be apparent from the figure now referred to. They obviously suspend the ovum, and retain it in its situation in the fluid of the Graafian vesicle; from which office, with others to be hereafter mentioned, I propose to name these structures the *Retinacula*†.

† The granulous cords extending from the membrana granulosa to the central mass are probably much more numerous at this period than represented in the figure; but the minuter ones could not in this instance be seen, nor are the minutest ever visible, from the exterior of the Graafian vesicle.

81. Every observer who has burst a Graafian vesicle under the microscope must have noticed the mass of granules that always escapes with the ovum. Von Baer, indeed, mentions "shreds" formed of granules as being visible in the mass thus escaping. The structures I have just mentioned (*retinacula*) (Plate VII. fig. 50. g^2.) explain the nature of the escaping mass of granules, and the "shreds" observed by Baer, which latter appear to have been the ruptured retinacula.

82. When describing the membrana granulosa (74.), I stated it to be sometimes of very unequal thickness, and to present villi projecting from its inner surface into the fluid of the Graafian vesicle. If Plate VII. fig. 50. g^2 and g^3 be again referred to, and the manner of origin, as I suppose, of the membrana granulosa borne in mind, the cause of those inequalities in the thickness of the latter will be evident. The villi on the internal surface of this membrane are nothing more than the remains of the retinacula, broken off as these are when the contents of a Graafian vesicle escape†.

83. The retinacula as now described, extending from the membrana granulosa to the centre of the Graafian vesicle, are in most animals very indistinctly seen, and hence it is that they have been hitherto overlooked. I first saw them in the Rabbit, and to those who may wish to convince themselves of their existence I recommend this animal, from the comparative distinctness with which they are here seen (Plate VII. fig. 50. g^2.). They are very distinct also in the Ferret (*Mustela Furo*); less so, yet visible, from the exterior of the ovisac in the Ruminantia (figs. 51 and 52. g^2.): and having never failed to find some trace of them, it is my opinion that these structures are general in the class Mammalia‡.

84. Very often when the central mass is visible, and the ovum distinct within it, nothing is seen of the cord-like portions of the retinacula. This is sometimes owing to the size of the Graafian vesicle rendering the object too remote, or to the imperfect transparency of its membranes, and sometimes to undue pressure having been applied, which breaks the cord-like portions down. Care must be taken, when they are sought for, to apply no more pressure than is absolutely needful; and perhaps the general remark may not here be out of place, that even with the most careful manipulation these parts may sometimes become displaced. Dissecting out a Graafian vesicle, stripping off the peritoneal covering of the ovary, or slicing and even handling the latter, tend to disturb the exquisitely suspended ovum.

The Ovum conveyed to the periphery of the Graafian Vesicle.

85. From the centre of the Graafian vesicle the ovum passes to its periphery. This change of place is gradual, and the same structures which we have seen to suspend the ovum in the former situation convey it to the latter. In Plate VII. fig. 55. this

† Although for the sake of perspicuity I have thought it best to distinguish the membrana granulosa and retinacula by two appellations, I think they are but parts of the same structure.

‡ I have seen these structures (as well as the tunica granulosa) in the Goat, the Hedgehog, and the Human subject.

change is seen in profile taking place. The granulous cords that are situated on one side of the central mass have in this instance disappeared, while those on the other side have become shortened, until the ovum has nearly reached the periphery of its Graafian vesicle. And what is very remarkable, and an interesting instance of *design*, the particular part of the periphery of the Graafian vesicle to which the ovum is thus conveyed, is always that directed towards the surface of the ovary.

The Ovum at the periphery of the Graafian Vesicle†.

86. Nor does the office of the retinacula end here. Having suspended the ovum in the centre, and conveyed it to the periphery of the Graafian vesicle, they now retain it in the latter situation (Plate VII. figs. 55 to 59. g^2.‡). The central mass in which the ovum is contained has become smaller, in many instances nearly of the size and form of the tunica granulosa (g^1), and in some animals appears more or less distinctly circumscribed by a membrane§, a continuation of which invests the cord-like, or at this period the band-like portions of the retinacula‖. The band-like portions of the retinacula have become reduced frequently to four in number, which are at right angles to each other (figs. 58 and 59. g^2.). Sometimes only two remain, and I have met with more than four (figs. 56 and 57. g^2.). The retinacula often seem closely pressed against the inner membrane of the Graafian vesicle, for which purpose, apparently, a portion of the membrana granulosa is in some instances previously removed (figs. 57 and 59. g^3.). In others the membrana granulosa is quite entire.

87. In Plate VII. fig. 61. is seen a fragment (g^2) of the central portion of the retinacula, from which the granulous bands have been broken off¶; and fig. 60. presents the same part *in situ*, and of the real size, as viewed in a large Graafian vesicle (*h i*) removed from the ovary, laid open, and its outer third reflected.

88. In the determinate pressure of the ovum towards the periphery of its Graafian vesicle, the retinacula obviously perform an important part††. One effect of this de-

† See the seventh column in the Table, page 304.

‡ One of the figures now referred to (fig. 58. from the Ferret,) presents a curious resemblance to the eye-ball with nearly all its parts, including the four *recti* muscles.

§ It is generally supposed that the surface of the "ovulum" (ovum) is in contact with the wall of its containing cavity; which however cannot be the case if the membrane now mentioned circumscribe the central portion of the retinacula on all sides.

In some animals that portion of this membrane which invests the central part of the retinacula is exceedingly distinct, as for instance in the Cat and Dog.

This membrane sometimes presents wrinkles or folds, as if susceptible of enlargement.

‖ It is not improbable that a continuation of this membrane, reflected from the retinacula, may invest the internal surface of the membrana granulosa (82. Note).

¶ Drawn as seen lying in a watch-glass.

†† In the Dog the retinacula generally become at this period very obscure (Plate VIII. fig. 66. g^3.), often quite imperceptible; and their central portion is reduced to a distinct membrane, closely investing the tunica granulosa (fig. 66. g^1. and g^2.); this membrane being, I presume, the "circular border" mentioned by Baer as circumscribing his "disc" (tunica granulosa) in this animal. That the retinacula, however, though generally indistinct, and indeed invisible, in this animal, continue to exercise an influence on the situation of the ovum,

terminate pressure appears to be penetration through the membrana granulosa, when the latter has not been previously removed. Another effect, probably in all instances, is the attenuation or removal, at one point, of the proper granulous tunic of the ovum (69.). By continued pressure, also, the retinacula may contribute to attenuate that part of the inner membrane (formerly the ovisac) of the Graafian vesicle in contact with which they lie†, and thus promote the expulsion of the ovum from the ovary.

89. All those who have been engaged in researches of this kind, must have noticed that on the bursting of an ovisac under the compressor, the ovum and a mass of granules are among the parts escaping first. This has been mentioned as remarkable. I think it will appear less so if the situation of the ovum as seen in most of the figures in Plate VII. be considered.

90. The whitish or yellowish white speck visible with the naked eye from the exterior of the ovary in the Dog, which led to Baer's discovery of his "ovulum" (ovum) of Mammalia, appears to be in this animal the tunica granulosa rather than the ovum. In some others in which such specks are visible from the exterior of the ovary, the central portion of the retinacula is also seen. This I find to be the case in the Rabbit and the Ferret, and in minute Graafian vesicles of the Ox. In the Rabbit I have with a good pocket lens discerned, from the exterior of the ovary, several of the *band-like portions* of the retinacula; and this I find possible in minute Graafian vesicles even of the Ox.

91. I have now gone through a minute description of the parts concerned in producing those appearances denominated by Von Baer the "cumulus" and "disc," as figured in Plate VII. fig. 62., and Plate VIII. fig. 80. If these figures, taken from that author‡, be contrasted with g^2 of my own figures in Plate VII., and particularly of fig. 55., it will be obvious that the "cumulus" of Baer is made up of what I have called the tunica granulosa, and the central portion of the retinacula; and that the band-like portions collectively of what I have called the retinacula, mainly contribute to present the appearance denominated the "flat disc" by Baer.

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is rendered probable by the fact, that when gentle compression has in some degree displaced this vesicle, it resumes its previous situation as soon as the pressure has been removed. This fact was observed by Professor Baer, but from not having seen the true form of the structures here called the retinacula, he was not in a condition to explain it.

One of the figures now referred to, from the Dog (Plate VIII. fig. 66.), presents four ova in one Graafian vesicle, which number, as well as three, I have repeatedly met with in this animal. Professor Valentin has also figured three in one Graafian vesicle of the Dog (Dissertation by Bernhardt, "Symbolæ," &c., fig. viii.). Two ova are very frequently present in the same Graafian vesicle in this animal; but in the Ferret (*Mustela Furo*) this is still more frequently the case. In the last-mentioned animal indeed I saw in many instances three, and not rarely four, ova in one Graafian vesicle. When this is the case, it is not unusual to find one or two ova apparently defective. To explain the existence of a plurality of ova in one Graafian vesicle, we must suppose that in such instances the membrane of the ovisac forms around, and thus includes in one cavity, the granulous envelopes of a corresponding number of germinal vesicles (23.).

† Von Baer, without having seen the peculiar structure of the retinacula, conjectured that this effect might be produced by the "ovulum" (ovum) on the parietes of its Graafian vesicle.

‡ Figs. ix. and xii. of Baer, Lettre, &c.

The Tunica granulosa and Retinacula of Mammals compared with the Membrana chalazifera and Chalazæ of Birds.

92. After what has been stated on the subject of the tunica granulosa and retinacula, it may by some be supposed that these structures are analogous to the membrana chalazifera and chalazæ of the Bird. They differ however in several respects. Thus the tunica granulosa and retinacula invest the *chorion*, while the membrana chalazifera invests the *proper membrane of the yelk*. The former exist *ante* coitum, the latter are formed *post* coitum. The retinacula and chalazæ may agree in the function of both having reference to the *position* of the ovum, but the *object* of the positions they respectively influence is essentially different, the one determining the situations of the ovum in the ovary, and assisting in its expulsion from that organ; the other (as it is supposed) performing a function which has reference to the parent heat in incubation.

Recapitulation.

93. In Mammalia and in Birds the germinal vesicle and its contents are those parts of the ovum which are first formed. This is probably the case in ova throughout the animal kingdom (12—22.).

94. The germinal vesicle at an early period is surrounded, in Mammalia and in Birds, by oil-like globules and peculiar granules, forming an envelope not hitherto described (14.).

95. The ovum of all vertebrated animals is contained in a vesicle, (the "chorion" of some authors, as found in Birds, Amphibia, and Fishes,) which is essentially the same in structure wherever found, and which it is desirable universally to denominate an *ovisac* (2. 47.). This vesicle is the "couche *interne*" of the Graafian vesicle of Mammalia as described by Baer (24. 25.).

96. The Graafian vesicle of the Mammalia is an ovisac that has acquired a proper covering or tunic, susceptible of becoming highly vascular, which covering is the "couche *externe*" of the Graafian vesicle as described by Baer (25.).

97. The ovisac of Birds, Amphibia, and Fishes ("chorion" of some authors) acquires in like manner a proper covering or tunic, susceptible of becoming highly vascular; and by the union of the ovisac with this covering, there is constituted a structure analogous to the Graafian vesicle of Mammalia (47.). The quantity of yelk in the former being large, that portion of the ovary which contains the structure here referred to (as analogous to the Graafian vesicle of Mammals) becomes pendent; and now the united coverings of the yelk-ball, viz. the ovisac, its proper tunic, the ovarian substance, and in some instances the peritoneal investment, are together called the *calyx* (32. 45. 47.). From this it will be obvious that the Graafian vesicle is not, as it has been supposed to be, a structure peculiar to Mammalia; nor is it correct to consider the Graafian vesicle of Mammalia analogous to the whole calyx of other animals (48.).

98. The ovisac has at first an elliptical or ellipsoidal form, becomes more spherical, and in certain Mammals somewhat tapered at one end (2. 34. 3.).

99. The structure of the ovisac in some of the Mammalia may be examined when it does not exceed in length the 50th or even the 100th part of a Paris line, that is, in the latter case, the 1125th of an English inch (4.).

100. Myriads of ovisacs with their contents are formed that never reach maturity (2.).

101. Some of the ovisacs which do not reach maturity are situated in the parietes of Graafian vesicles in Mammalia, or of the corresponding structures in other Vertebrata; being sometimes formed in this situation, and sometimes included by the covering which the larger ovisac acquires. The minute ovisacs so situated it is proposed to denominate *parasitic* ovisacs (57.).

102. The ovisac is often found in a cavity proper to itself, with the walls of which it has no organic union (6.).

103. The granules forming the envelope of the germinal vesicle above referred to, and subsequently found in the fluid of the ovisac, are very peculiar in their appearance; they are generally of an elliptic form and flattened, highly transparent, contain a nucleus and sometimes a pellucid fluid also, and are intimately connected with the evolution of the ovum (8. 10.). These granules are present in largest quantity in the ovisac of Mammalia; yet granules essentially the same exist at an early period in the ovisac of Birds (32. 33.), and are sometimes met with in that of Fishes (34. 42.).

104. A continual disappearance of ova, and the formation of others, are observable even at a very early age (60.).

105. The ovum of Mammalia when completely formed is at first situated in or near the *centre* of the ovisac (75.).

106. It is at this period supported in the centre of the ovisac by an equable diffusion of granules throughout the fluid of the latter (76.).

107. The ovisac about the same time begins to acquire its proper covering or tunic, by which addition, as already stated, there is constituted a Graafian vesicle, and of the latter the ovisac is now the inner membrane. After this period, therefore, it is proper to speak, not of an ovisac, but of a Graafian vesicle (77.).

108. The peculiar granules of the Graafian vesicle arrange themselves to form three structures, viz. the *membrana granulosa* of authors, and two structures not hitherto described, one of which it is proposed to name the *tunica granulosa*, and the other, which is rather an assemblage of structures than a single structure, the *retinacula* (78. 79. 82—Note.).

109. The tunica granulosa is a spherical covering proper to the ovum, and explains why the outer line in the double contour of the thick chorion, that is, the external surface of this membrane, remained so long unobserved. At a certain period, this tunic, in some animals at least, is seen to have tail-like appendages consisting of granules similar to its own (64—71.).

110. The retinacula consist of a central mass containing the ovum in its tunica granulosa, and of cords or bands extending from this central mass to the membrana granulosa. These structures at a certain period become invested by a membrane (80—84.).

111. The office of the retinacula appears to be, first, to suspend the ovum in the fluid of the Graafian vesicle; next, to convey it to a certain part of the periphery of this vesicle; subsequently to retain it in the latter situation; and finally, to promote its expulsion from the ovary (80—88.).

112. The particular part of the periphery of the Graafian vesicle to which the ovum is conveyed, is uniformly that directed towards the surface of the ovary (85.).

113. The mass of granules escaping with the ovum on the bursting of a Graafian vesicle under the compressor, is composed chiefly of the tunica granulosa and the ruptured retinacula (64. 81.).

114. The "cumulus" of Professor Baer is made up of the parts here called the tunica granulosa and the central portion of the retinacula; and the band-like portions, collectively, of what are here called the retinacula, mainly contribute to produce the appearance denominated the "flat disc" by that author (91.).

115. In Mammals a thick and highly transparent membrane, the true chorion, is formed external to the proper membrane of the yelk, while the latter is in the ovary. The inner part of the substance of the chorion in its early stages is in a fluid state, but it subsequently acquires more consistence. There is not any corresponding structure *within the ovary* of other vertebrated animals; so that the body expelled from that organ in the latter, is not (as in Mammalia) a true ovum, but a yelk-ball (52—56.).

116. The following appears to be the order of formation, as to time, of the more permanent parts of the ovum and the Graafian vesicle, in Mammalia, viz.

1. The germinal vesicle, with its contents (12—22.).
2. An envelope consisting of oil-like globules and peculiar granules (14—16.).
3. The ovisac, which forms around this envelope (23.).
4. The yelk, which forms within the ovisac around the germinal vesicle (24.).
5. The proper membrane of the yelk, which makes its appearance while the yelk is still in an incipient state (24. 49.).
6. The chorion (24. 52.).
7. The proper covering or tunic of the ovisac (77.); and about the same time the peculiar granules of the ovisac arrange themselves (78. 79.) to form,
   - The tunica granulosa (64.),
   - The retinacula (80.), and
   - The membrana granulosa (72.).

117. Such of these structures as are present in other vertebrated animals originate in the same order as to time (27—47.).

118. *Table of Measurements.*

The measurements are given in fractions of a Paris line (‴), the micrometer used, one of Frauenhofer's, being divided according to French measure. The French inch (of twelve lines) is to the English inch, as 1·06575 is to 1·00000, or nearly one fifteenth more. Assuming it to be exactly one fifteenth more, the simplest mode of converting the fraction of a French *line* into the fraction of an English *inch*, will be to multiply the denominator of the former by the number 11·25 (or $11\frac{1}{4}$). Thus, the actual length of the smallest ovisac in fig. 9. from the Dog, measuring $\frac{1}{100}$th of a Paris line, is found to be $\frac{1}{112\frac{1}{2}}$th of an English inch.

The compressor having been generally used in these researches, some allowance must be made on this account, the actual size of the objects *h* and *h i* being rather below the measurements given in the Table.

When the object is elliptical, it is the long diameter the measurement of which is given in the Table.

No. of Figure.	Animal.	Diameters magnified.	Actual Dimensions. *b.* Germinal spot.	*c.* Germinal vesicle.	*f.* Chorion (ovum).	*g*¹. Tunica granulosa.	*h.* Ovisac.	*h i.* Graafian vesicle, or corresponding structure.
1.	Rabbit.....	440	$\frac{1}{250}$	$\frac{1}{150}$ to $\frac{1}{100}$				
2†.	Cat	440						
3‡.	Cat	440						
4.	Ox (Heifer)....	100	.....	.....	.....	.....	$\frac{1}{50}$ to $\frac{1}{25}$, majority about $\frac{1}{30}$.	
5.	Sheep	60	.....	.....	.....	.....	$\frac{1}{25}$ to $\frac{1}{20}$, $\frac{1}{}$	
6.	Cat	100	.....	.....	.....	.....	$\frac{1}{50}$ and upwards.	
8.	Cat	440	.....	.....	.....	.....	$\frac{1}{50}$	
9.	Dog	440	.....	.....	.....	.....	$\frac{1}{150}$ to $\frac{1}{100}$	
10.	Cat	240	.....	.....	.....	.....	$\frac{1}{50}$ to $\frac{1}{30}$	
11§.	Man.....	440	.....	.....	.....	.....	$\frac{1}{50}$	
12.	Man.....	150	.....	.....	.....	.....	$\frac{1}{50}$ and upwards.	
13.	Cat	440	$\frac{1}{250}$	$\frac{1}{150}$	.....	.....	$\frac{1}{50}$	
14.	Hog	240	.....	$\frac{1}{150}$	.....	.....	$\frac{1}{50}$	
15.	Hog	240	.....	.....	.....	.....	$\frac{1}{50}$	
16.	Hog	240	.....	.....	.....	.....	$\frac{1}{30}$	
17‖.	Cat	440	$\frac{1}{250}$	$\frac{1}{150}$	$\frac{1}{30}$	.....	$\frac{1}{}$	
22.	Pigeon	440	.....	$\frac{1}{250}$ to $\frac{1}{150}$	.....	.....	$\frac{1}{50}$ to $\frac{1}{30}$	
23.	Common Fowl..	440?	.....	$\frac{1}{250}$	.....	.....	$\frac{1}{50}$	

† The whole object $\frac{1}{50}$‴. ‡ The whole object $\frac{1}{50}$‴. § The peculiar granules $\frac{1}{150}$‴.
‖ Oil-like globules, some of them $\frac{1}{250}$‴ and upwards. The peculiar granules, mean length $\frac{1}{150}$‴.

Table of Measurements. (Continued.)

No. of Figure.	Animal.	Diameters magnified.	Actual Dimensions.					
			b. Germinal spot.	*c.* Germinal vesicle.	*f.* Chorion (ovum).	g^1. Tunica granulosa.	*h.* Ovisac.	*h i.* Graafian vesicle, or corresponding structure.
24.	Common Fowl..	440	.....	.....	.....	.....	$\frac{1}{1\,0}$	
25.	Common Fowl..	440	.....	.....	.....	.....	$\frac{1}{1\,0}$	
26.	Common Fowl..	100	.....	.....	.....	.....	.....	$\frac{1}{2}$
27.	Frog.....	?	.....	.....	.....	.....	$\frac{1}{2\,0}$	
28†.	Frog.....	100	.....	$\frac{1}{1\,0}$ to $\frac{1}{1\,0}$	.....	.....	$\frac{1}{4}$	
29.	Frog.....	60	.....	$\frac{1}{4}$	.....	.....	$\frac{1}{2}$	
30.	Salmon	240	.....	$\frac{1}{3\,0}$ and less				
31.	Cod	100	.....	$\frac{1}{1\,0\,0}$ to $\frac{1}{3\,0}$	.....	.....	$\frac{1}{2\,0}$ to $\frac{1}{1\,0}$	
32.	Whiting	100	.....	$\frac{1}{3\,0}$ to $\frac{1}{1\,0}$	.....	.....	$\frac{1}{4}$ to $\frac{1}{4}$	
33.	Haddock	100	.....	$\frac{1}{8\,0}$	.....	.....	$\frac{1}{1\,0}$	
34.	Turbot	240	.....	$\frac{1}{7\,0}$	.....	.....	$\frac{1}{1\,0}$	
35.	Salmon	240	.....	.....	.....	.....	$\frac{1}{1\,0}$ to $\frac{1}{4}$	
36.	Salmon	100?	.....	.....	.....	.....	.....	$\frac{1}{2}$
37.	Salmon	25	.....	.....	.....	.....	.....	$\frac{1}{2}$
39.	Dog	60	.....	.....	$\frac{1}{7}$	$\frac{1}{7}$	.....	$\frac{1}{2}$
40.	Mole	100	.....	.....	$\frac{1}{7}$	$\frac{1}{7}$	.....	$\frac{1}{2}$
41.	Mole	440	$\frac{1}{1\,4\,0}$	$\frac{1}{7\,0}$	$\frac{1}{3\,0}$	$\frac{1}{1\,0}$	$\frac{1}{4}$	
42.	Mole	240	.....	$\frac{1}{8\,0}$	$\frac{1}{1\,0}$	$\frac{1}{1\,0}$		
43.	Sheep	240	$\frac{1}{1\,4\,0}$	$\frac{1}{4\,0}$	$\frac{1}{1\,0}$	$\frac{1}{1\,0}$		
44.	Dog	440	.....	.....	$\frac{1}{1\,0}$	$\frac{1}{4}$		
45.	Hog	150	.....	.....	$\frac{1}{2\,0}$	$\frac{1}{1\,0}$		
46.	Hog	100	.....	.....	$\frac{1}{1\,0}$	$\frac{1}{1\,0}$		
47.	Dog	150	.....	.....	$\frac{1}{1\,0}$	$\frac{1}{1\,0}$		
64.	Hog	100	.....	.....	.....	.....	.....	$\frac{1}{4}$
65.	Mouse	100	.....	.....	$\frac{1}{3\,0}$	.....	.....	$\frac{1}{1\,0}$
66.	Dog	100	.....	.....	$\frac{1}{3\,0}$ to $\frac{1}{1\,0}$	.....	.....	$\frac{1}{4}$
67‡.	Rabbit	100	$\frac{1}{4\,0}$	$\frac{1}{1\,0}$	$\frac{1}{4}$	.....	.....	$\frac{1}{4}$
68.	Dog	?	.....	.....	.....	.....	$\frac{1}{3\,0}$ to $\frac{1}{4}$	$\frac{1}{4}$ to $\frac{1}{4}$
69.	Sheep	60	$\frac{1}{1\,4\,0}$	$\frac{1}{1\,0}$	$\frac{1}{3\,0}$	.....	$\frac{1}{4}$	
71§.	Hog	100						
74.	Hog	60	.....	.....	$\frac{1}{3\,0}$ to $\frac{1}{3\,0}$	.....	$\frac{1}{4}$ to $1\frac{1}{4}$	
75.	Dog	100	.....	.....	.....	.....	$\frac{1}{3\,0}$ to $\frac{1}{3\,0}$	$\frac{1}{4}$
76.	Pigeon.....	60	.....	.....	.....	.....	$\frac{1}{3\,0}$	$\frac{1}{4}$
79‖.	Ox (Heifer)....	25						

† The spots, marked *d'*, $\frac{1}{1\,0}'''$.

‡ The ovum in this instance was enlarged, and probably in the course of being absorbed.

§ g^3, Membrana granulosa $\frac{1}{1\,0}'''$ in thickness.

‖ *h*, Proper membrane of the parasitic ovisac (incipient Graafian vesicle?) $\frac{1}{4}'''$ in thickness; its cavity $1\frac{1}{4}'''$ in length when crushed.

119. Explanation of the Plates.

N.B. In all the figures the same letters denote the same objects, the alphabetical order of these letters corresponding with the order of position of the parts in proceeding outwards from the centre of the ovum: viz.

a. Fluid, or granulous fluid, of the germinal vesicle.
b. Germinal spot or germinal spots.
c. Germinal vesicle.
d. Yelk.
d'. Spot on the internal surface of the membrana vitelli in the Frog.
e. Membrana vitelli.
f. Chorion,—also ovum.
g. Peculiar granules, first constituting the envelope of the germinal vesicle, next contained in the fluid of the ovisac, and lastly arranging themselves to form:
g^1. Tunica granulosa.
g^2. Retinacula.
g^3. Membrana granulosa.
h. Ovisac, or proper membrane of the ovisac.
i. Proper covering or tunic of the ovisac,—also blood-vessels.
h i. Graafian vesicle, or corresponding structure (capsule) (consisting of $h + i$).
k. Stroma, Parenchyma, substance in which the ovisac is imbedded, or couch.
l. Peritoneum.
i k l. Pedicle (formed by the union of *i*, *k* and *l*).
xx. "Stigmate" of Baer.
* Oil-like globules.
g*. Isolated spots (consisting chiefly of $g + *$).

PLATE V.

Fig. 1. Rabbit (*Lepus Cuniculus*, Linn.). Very early stages in the formation of the ovum (14.). *c*. The germinal vesicle containing (*a*) fluid, and in one instance (*b*) a well-marked spot. These germinal vesicles measured from $\frac{1}{150}'''$ to $\frac{1}{250}'''$ (Paris line) in their long diameter. *g*. An envelope of peculiar granules (14.). The ovisac is not yet formed. Compare with fig. 2. from the Cat, and figs. 19. and 20. from the Pigeon. 440 *diameters*.

Fig. 2. Cat (*Felis Catus*, Linn.). Stages in the formation of the ovum (16.) nearly corresponding to some of those seen in fig. 1., but the germinal vesicle in this instance is concealed (12. 24.). *g*. Envelope of peculiar granules;

*. oil-like globules added to the envelope of granules: these objects measured $\frac{1}{75}'''$ (Paris line) in length. 440 *diam.*

Fig. 3. Cat (*Felis Catus*, Linn.). Stages more advanced (16.). These objects measured $\frac{1}{150}'''$ (Paris line) in length. 440 *diam.*

Fig. 4. Ox (*Bos Taurus*, Linn.). More than a hundred parasitic ovisacs (*h*) (57.) in the field of view $\frac{1}{60}'''$ to $\frac{1}{45}'''$ (Paris line) (4.). In several of these ovisacs the germinal vesicle is visible. 100 *diam.*

Fig. 5. Sheep (*Ovis Aries*, Linn.). Ovisacs $\frac{1}{75}'''$ to $\frac{1}{150}'''$ (Paris line); one larger, $\frac{1}{3}'''$, containing an ovum. *k*. Stroma. 60 *diam.*

Fig. 6. Cat (*Felis Catus*, Linn.). Ovisacs $\frac{1}{300}'''$ (Paris line) and upwards. 100 *diam.*

Fig. 7. Ditto. Some of the ovisacs seen in the last figure more highly magnified. *g*. Peculiar granules; *b*. germinal spot; *c*. germinal vesicle.

Fig. 8. Ditto. Two of the same ovisacs still more magnified. 440 *diam.*

Fig. 9. Dog (*Canis familiaris*, Linn.). Ovisacs $\frac{1}{100}'''$ and upwards (in outline only). *g*. Peculiar granules, nearly as large as some of their containing ovisacs; *h*. proper membrane of the ovisac, very thick, and presenting folds in its contour (2. 7.). 440 *diam.*

Fig. 10. Cat (*Felis Catus*, Linn.). Parasitic ovisacs (57.) $\frac{1}{300}'''$ to $\frac{1}{175}'''$. *h*. Proper membrane of the ovisac (7.); *g*. peculiar granules; *c*. germinal vesicle. Compare with fig. 21. from the Pigeon, and fig. 32. Plate VI. from the Whiting. 240 *diam.*

Fig. 11. Man. Ovisac $\frac{1}{45}'''$. *g*. Peculiar granules; *h*. proper membrane of the ovisac. 440 *diam.*

Fig. 12. Ditto. Ovisacs $\frac{1}{45}'''$, and upwards; the minuter ones more spherical (3.) than the one in the last figure. The germinal vesicle (*c*) is visible in the largest of the ovisacs. 150 *diam.*

Fig. 13. Cat (*Felis Catus*, Linn.). Parasitic ovisacs (57.): the smallest $\frac{1}{300}'''$, the largest $\frac{1}{75}'''$. *b*. Germinal spot; *c*. germinal vesicle; *. oil-like globules; *h*. proper membrane of the ovisac. 440 *diam.*

Fig. 14. Hog (*Sus Scrofa*, Linn.). Ovisac $\frac{1}{125}'''$. *. Oil-like globules surrounding the minute germinal vesicle; *g*. peculiar granules; *h*. proper membrane of the ovisac. 240 *diam.*

Fig. 15. Ditto. Incipient yelk (oil-like globules (*) and fluid). The germinal vesicle is concealed. *g*. Peculiar granules; *h*. proper membrane of the ovisac. Compare with figs. 22. 23. and 24. from Birds. 240 *diam.*

Fig. 16. Ditto. Membrana vitelli (*e*) partly visible. *. Oil-like globules, some of which closely surround and conceal the minute germinal vesicle; *h*. ovisac. Compare with fig. 25. from the common Fowl; fig. 34. Plate VI. from the Turbot; and fig. 35. Plate VI. from the Salmon. 240 *diam.*

Fig. 17. Cat (*Felis Catus*, Linn.). Thick and transparent chorion (*f*) just formed (52—56.). *e*. Membrana vitelli almost concealed by the chorion (the

separate existence of the former is seen in two places (49.)); *b*. germinal spot; *c*. germinal vesicle; *. oil-like globules in the yelk, which consisted besides of a pellucid fluid and minute granules; *g*. peculiar granules; *h*. ovisac. No appearance yet of either the tunica granulosa (64.), the retinacula (80.), the membrana granulosa (72.), or the proper covering of the ovisac (77.). 440 *diam.*

Fig. 18. Pigeon (*Columba livia*, Linn.). A very early stage in the formation of the ovum (15.). *c*. Germinal vesicle; *a*. its contents; *. oil-like globules†. Compare with fig. 30. Plate VI. from the Salmon. 440 *diam.*

Fig. 19. Ditto. This figure corresponds to fig. 1. from the Rabbit, and fig. 2. from the Cat (15.). *g*. Envelope of peculiar granules; *c*. germinal vesicle; *. oil-like globules†. 440 *diam.*

Fig. 20. Ditto. Proper membrane of the ovisac (*h*.) in the course of formation around (*g*.) the envelope of peculiar granules (23.). This appears to be the stage succeeding that represented by the larger objects in fig. 1. from the Rabbit, and by those in fig. 3. from the Cat†. 440 *diam.*

Fig. 21. Ditto. A multitude of ovisacs. Compare with fig. 10. from the Cat, and fig. 32. Plate VI. from the Whiting. 150 *diam.*

Fig. 22. Ditto. Incipient yelk (24.), consisting of oil-like globules (*) and a pellucid fluid. *c*. Germinal vesicle; *h*. ovisac. Compare with fig. 15. from the Hog. 440 *diam.*

Fig. 23. Common Fowl (*Phasianus Gallus*, Linn.). Incipient yelk (24.), consisting of oil-like globules (*) and a pellucid fluid. *c*. Germinal vesicle; *g*. peculiar granules; *h*. ovisac, $\frac{1}{400}'''$. Compare with fig. 15. from the Hog. 440 (?) *diam.*

Fig. 24. Ditto. The yelk (*d*) just formed (consisting of oil-like globules and a fluid), but the membrana vitelli not visible. *h*. Ovisac, $\frac{1}{175}'''$. The fluid of the ovisac is obscurely granulous. 440 *diam.*

Fig. 25. Ditto. Membrana vitelli (*e*) now visible. The yelk (*d*) consists still for the most part of oil-like globules and a pellucid fluid. *h*. Ovisac. Compare with fig. 16. from the Hog; fig. 34. Plate VI. from the Turbot; and fig. 35. Plate VI. from the Salmon. 440 *diam.*

Fig. 26. Ditto. An ovisac acquiring a proper covering or tunic, the union constituting the structure *h i* (47.), still very transparent. The yelk is seen escaping through the pedicle, which in this instance it has ruptured (32 Note). *e*. Membrana vitelli; *g*. peculiar granules. 100 *diam.* See the ideal section Plate VI. fig. 38½.

Fig. 26½. Cat (*Felis Catus*, Linn.). Oil-like globules (*) accumulated on the peculiar granules (*g*) of the ovisac. (Plate VIII. fig. 66. presents the same objects in less number, and therefore appearing as isolated spots, *g**).

† The objects represented in figs. 18. 19. and 20. were found lying among ovisacs apparently just formed.

PLATE VI.

Fig. 27. Frog (from R. Wagner). *h*. Ovisac, $\frac{1}{10}'''$, ("chorion" of Wagner (34.)); *b*. germinal spots, *many* in the *naked* Amphibia (R. Wagner); *c*. germinal vesicle; *e*. membrana vitelli; *d*. yelk, pellucid at this period.

Fig. 27½. *Coluber Natrix* (from R. Wagner). *b*. Germinal spot, *single* in the *squamous* Amphibia (R. Wagner); *c*. germinal vesicle; *d*. yelk, in its membrane; *h*. ovisac ("chorion" of Wagner (34.)).

Fig. 28. Frog (*Rana temporaria*, Linn.). A spot (*d'*), generally elliptical, and in this instance $\frac{1}{15}'''$ in length, which I always find on the internal surface of the membrana vitelli of the Frog in ovisacs of about this size ($\frac{1}{3}'''$) (40.). *b*. Germinal spots; *c*. germinal vesicle; *d*. yelk; *e*. membrana vitelli; *h*. ovisac. 100 *diam*.

Fig. 29. Ditto. The membrana vitelli (*e*) rendered visible by maceration†. *c*. The germinal vesicle, indistinctly seen in the centre of (*d*) the yelk; *e*. membrana vitelli; *h*. ovisac. 60 *diam*.

Fig. 30. Salmon (*Salmo Salar*, Linn.). A very early stage in the formation of the ovum, nearly corresponding to figs. 18. and 19. Plate V. from the Bird, and to figs. 1. 2. and 3. Plate V. from Mammals (17.). *c*. Germinal vesicles, the largest $\frac{1}{10}$th of a Paris line in diameter; their contents consisting chiefly of minute oil-like globules of a yellowish colour. Compare these vesicles with the germinal vesicles in fig. 35. from the same fish. One of the germinal vesicles in fig. 30. has another vesicle external to it. Is this a minute ovisac just formed? (See the manner of origin of the ovisac (23.)). 240 *diam*.

Fig. 31. Cod (*Gadus Morrhua*, Linn.). Ovisacs, $\frac{1}{15}'''$ to $\frac{1}{10}'''$; the contour in folds (34.). The yelk is not yet visible, being pellucid at this period. *b*. Germinal spots, *many* in osseous Fishes (R. Wagner); *c*. germinal vesicle, its fluid tinged with yellow; *. oil-like globules; *h*. ovisac. 100 *diam*.

Fig. 32. Whiting (*Gadus Merlangus*, Linn.). Ovisacs $\frac{1}{4}'''$ to $\frac{1}{3}'''$; the contour in folds (34.). *c*. Germinal vesicle; *g*. peculiar granules in the fluid of the ovisac (8.); *h*. proper membrane of the ovisac; *d*. incipient yelk (24.). 100 *diam*.

Fig. 33. Haddock (*Gadus Æglefinus*). The yelk (*d*) now visible as well as its proper membrane (*e*). *b*. Germinal spots; *c*. germinal vesicle; *h*. proper membrane of the ovisac. 100 *diam*.

Fig. 34. Turbot (*Pleuronectes maximus*, Linn.). Membrana vitelli (*e*) now visible. *c*. Germinal vesicle; *h*. ovisac; *d*. the yelk, consisting of oil-like globules immediately around the germinal vesicle, and of granules on the internal

† As recommended by R. Wagner.

surface of the membrana vitelli. Compare with fig. 25. Plate V. from the common Fowl, and fig. 16. Plate V. from the Hog. 240 *diam.*

Fig. 35. Salmon (*Salmo Salar*, Linn.). Ovisacs and their contents in nearly the same state as those in the last figure. *g*. Some of the peculiar granules (8.) are seen in the fluid of the ovisac (34.). Compare with fig. 25. Plate V. from the common Fowl, and fig. 16. Plate V. from the Hog.

Fig. 36. Ditto. The ovisac (*h*) acquiring a proper covering or tunic (*i*) (47.). In this instance two minute ovisacs are included by the covering (57.), their situation here being within the incipient pedicle. 100 (?) *diam.*

Fig. 37. Ditto. The same objects (*h i*) in a more advanced stage, but the covering still very transparent. The vesicles have been crushed, and the yelk is seen escaping through the pedicles, taking with it its proper membrane (*e*). 25 *diam.* See the ideal section, Plate VI. fig. 38½.

Fig. 38. Ditto. The perfect calyx with its pedicle. 25 *diam.*

Fig. 38½. Bird, &c. Ideal perpendicular section of that portion of the ovary which is called the calyx, and its contents. *d*. The yelk surrounded by its proper membrane, *e*; *h*. lining membrane of the calyx. (This lining membrane of the calyx was originally the independent ovisac.) *i*. Vascular covering or tunic of *h*. (By the union of *h* and *i* there is constituted *h i*, that portion (capsule) of a calyx which is analogous to the Graafian vesicle of Mammals.) *k*. Substance of the ovary; *l*. peritoneum; *i*, *k*, and *l* are reflected off from *h*, thus forming a pedicle *i k l*. This pedicle is comparatively hollow, so that when the calyx is crushed, a portion of the yelk (meeting with least resistance at the point where the reflection above-mentioned takes place,) escapes into it.

Fig. 39. Dog (*Canis familiaris*, Linn.). Granules (*g*) of the Graafian vesicle circumscribed by a distinct line (central portion of the retinacula?). *g**. Isolated spots; g^3. membrana granulosa; g^1. tunica granulosa; *h i*. Graafian vesicle. 60 *diam.*

Fig. 40. Mole (*Talpa europæa*, Linn.). The same objects with the exception of *g**.

Fig. 41. Ditto. The tunica granulosa (g^1) in the course of formation (68.). *h*. Ovisac; *i*. blood-vessels; *b*. germinal spot (of a yellowish colour); *c*. germinal vesicle; *d*. yelk; *f*. chorion. 440 *diam.*

Fig. 42. Ditto. The tunica granulosa (g^1) removed entire from the Graafian vesicle. *c*. Germinal vesicle in the centre of (*d*) the yelk; *f*. chorion.

Fig. 43. Sheep (*Ovis Aries*, Linn.). The tunica granulosa removed almost entire from the ovisac. *b*. Germinal spot; *c*. germinal vesicle; *d*. yelk; *f*. chorion. 240 *diam.*

Fig. 44. Dog (*Canis familiaris*, Linn.). The tunica granulosa (g^1) very highly magnified; most of its granules presenting very distinctly a nucleus, and some of them being very globular in form (9.). *d*. Yelk; *f*. chorion. At

one part of the surface of the yelk is seen a pellucid space, which is the situation of the germinal vesicle. This figure presents the state of the ovum when absorption is commencing (60. Note.). 440 *diam.*

Fig. 45. Hog (*Sus Scrofa*, Linn.). The tunica granulosa (g^1) removed entire from the ovisac, with some adherent granules, which form part of the ruptured retinacula (g^2). *b.* Germinal spot; *c.* germinal vesicle; *d.* yelk; *f.* chorion. 150 *diam.*

Fig. 46. Ditto. The tunica granulosa (g^1) relatively thicker than the last, from a loosening of its granules by incipient liquefaction (g^2). Adherent granules of the ruptured retinacula. A pellucid space is seen at one part of the surface of (*d*) the yelk, which is the situation of the germinal vesicle. This figure presents an early stage in the absorption of the ovum (60. Note.).

Fig. 47. Dog (*Canis familiaris*, Linn.). The tunica granulosa (g^1) removed entire from the ovisac, with part of the ruptured retinacula (g^2) adherent. *d.* yelk; *f.* chorion. 150 *diam.*

PLATE VII.

Fig. 48. Dog (*Canis familiaris*, Linn.). Incipient separation of the granules from the fluid of the ovisac (78.). g^2. First appearance of the retinacula; *d.* yelk, the only portion of the ovum in this instance visible from the exterior of (*h i*) the Graafian vesicle. 60 *diam.*

Fig. 49. Guinea Pig (*Cobaya Aperea*, Cuv.). The same objects in a stage more advanced (78.). g^1. Tunica granulosa. 150 *diam.*

Fig. 50. Rabbit (*Lepus Cuniculus*, Linn.). A stage still more advanced (79.). The peculiar granules of the Graafian vesicle (*h i*) are now seen to have become arranged into three incipient structures, viz. g^1. the tunica granulosa (64.); g^2. the retinacula (80.); g^3. the membrana granulosa (72.); *f.* the chorion. 100 (?) *diam.*

Fig. 51. Sheep (*Ovis Aries*, Linn.). The retinacula (g^2) supporting the ovum (*f*) in the centre of (*h i*) the Graafian vesicle (80.). The tunica granulosa, though probably formed, is not distinctly visible. 60 *diam.*

Fig. 52. Ox (*Bos Taurus*, Linn.). The same objects, together with (g^1) the tunica granulosa. 100 *diam.*

Fig. 53. Ditto. The retinacula (g^2) removed from the Graafian vesicle. g^1. Tunica granulosa; *f.* chorion (ovum). 100 *diam.*

Fig. 54. Dog (*Canis familiaris*, Linn.). The same objects, though in this figure the membrane of the central portion of the retinacula (g^2) is closely applied to, and hence scarcely distinguishable from, the tunica granulosa (g^1) (88. Note). *c.* Germinal vesicle in the centre of the yelk. 100 *diam.*

Fig. 55. Ox (*Bos Taurus*, Linn.). The ovum (*f*) approaching the periphery of

(*h i*) the Graafian vesicle, being conveyed by (g^2) the retinacula. g^1. Tunica granulosa; g^3. membrana granulosa. Contrast with fig. 62. Plate VII. and with fig. 80. Plate VIII. from Baer (62. 63. 91.). The "cumulus" of Baer (central portion of the retinacula) is well seen in this figure. 60 *diam.*

Fig. 56. Ox (*Bos Taurus*, Linn.). The ovum (*f*) (in its tunica granulosa) at the periphery of (*h i*) the Graafian vesicle, being retained there by (g^2) the retinacula (86.). The other objects as in the last figure. It is the *under* surface of the ovum, &c. that is here seen. 60 *diam.*

Fig. 57. Dog (*Canis familiaris*, Linn.). The ovum (*f*) apparently just reaching the periphery of (*h i*) (a portion of) the Graafian vesicle. g^2. Retinacula; g^1. tunica granulosa; g^3. membrana granulosa, part of which has been removed (86.). 100 *diam.*

Fig. 58. Ferret (*Mustela Furo*, Linn.). The ovum (*f*) at the periphery of (*h i*) the Graafian vesicle. g^2. Retinacula (80.); g^1. tunica granulosa; *c.* germinal vesicle. 100 *diam.*

Fig. 59. Rabbit (*Lepus Cuniculus*, Linn.). The same objects. g^3. Membrana granulosa, part of which has been removed (86.).

Fig. 60. Ox (*Bos Taurus*, Linn.). The under surface of the central portion of the retinacula (g^2) ("*cumulus*" of Baer) at the periphery of (*h i*) the Graafian vesicle. *k.* Adherent stroma.

Fig. 61. Ditto. Fragment of the central portion of the retinacula (g^2), from which the granulous bands have been broken off, yet still containing the ovum in its tunica granulosa (g^1); the latter adherent to the walls of the cavity in which it lies. (The drawing was taken as the object lay under the microscope in a watch-glass.)

Fig. 62. From Baer, "Vésicule de De Graaf (24. 25.) (de grandeur moyenne) d'une truie, grossie dix fois; dissequée suivant son axe.

1. Surtout péritonéal (*l*).
2. Tissu cellulaire (le *stroma*) (*k*).
3. Couche externe (*i*)
4. Couche interne (*h*)

} de la capsule de la vésicule de De Graaf.

x. x. Stigmate

5. Membrane granuleuse (g^3)
6. Fluide contenu
7. Disque proligère (g^2)
8. Ovule (*f*)

} du noyau de la vésicule de De Graaf."

This figure, from Baer, is lettered so as to correspond with the other figures.

Fig. 63. Rabbit (*Lepus Cuniculus*, Linn.). The retinacula (g^2), with their membrane, removed from the Graafian vesicle. g^1. Tunica granulosa, with its tail-like appendages (65.); *f.* chorion (ovum); *d.* yelk; *c.* germinal vesicle; *b.* germinal spot. 240 *diam.*

PLATE VIII.

Fig. 64. Hog (*Sus Scrofa*, Linn.). The ovum in its tunica granulosa (g^1) at the periphery of (*h i*) the Graafian vesicle, retained in this situation by (g^2) the retinacula (86.). 100 *diam.*

Fig. 65. Mouse (*Mus Musculus*, Linn.). The same objects; but the ovum (*f*) in this instance has not reached the periphery of (*h i*) the Graafian vesicle. *c*. The germinal vesicle, relatively very large; *b*. the germinal spot.

Fig. 66. Dog (*Canis familiaris*, Linn.). Four ova in one Graafian vesicle (88. Note), and at or near to its periphery. g^2. Retinacula; the central portion of the retinacula (g^2) reduced to little more than a membrane, which closely surrounds (g^1) the tunica granulosa, and appears to be the cause of the well-defined contour of the latter, as seen *in situ* in this animal (88. Note). g^*. Isolated spots (59.), consisting chiefly of extremely minute oil-like globules accumulated on the peculiar granules of the Graafian vesicle. In this instance these spots were seen to be most numerous around some of the ova. (Plate V. fig. 26½. presents these spots in close aggregation.)

Fig. 67. Rabbit (*Lepus Cuniculus*, Linn.). An appearance frequent in this animal, probably denoting a stage in the absorption of the ovum (60.). *f*. Distended chorion; *d*. altered yelk; *c*. the enlarged germinal vesicle; *b*. the enlarged germinal spot; *h i*. Graafian vesicle. (The tunica granulosa and retinacula have disappeared.) 100 *diam.*

Fig. 68. Dog (*Canis familiaris*, Linn.). A patch of ovisacs (*h*) $\frac{1}{18}'''$ to $\frac{1}{4}'''$ in length, some of them passing into the state of Graafian vesicles, and among these two (*h i*) that have become opake from the coverings acquired. One of the ovisacs contains two ova (88. Note). *g*. Granules of the ovisac (incipient retinacula?).

Fig. 69. Sheep (*Ovis Aries*, Linn.). The cavity in which the ovisac is often found (6.). The ovisac (*h*) having been ruptured, the ovum (*f*) has escaped, and is seen to have become flattened by pressure. The ovum bursting before its exit, left behind its germinal vesicle (*c*). *b*. Germinal spot; *g*. peculiar granules of the ovisac; *k*. substance in which the ovisac is imbedded. 60 *diam.*

Fig. 70. Cat (*Felis Catus*, Linn.). The chorion (*f*) not yet of such consistence as to resist even very gentle pressure, which, applied on one side, has forced the now semifluid substance of the chorion to the other side (53.). *e*. Membrana vitelli (49. 53.); *d*. the yelk in an incipient state. 440 *diam.*

Fig. 71. Hog (*Sus Scrofa*, Linn.). Portion of the membrana granulosa (g^3) removed from the Graafian vesicle (73.). 100 *diam.*

Fig. 72. Man (from Bernhardt). An ovum after maceration (50. 56.). *d*. The yelk; *f*. the chorion. The internal surface of the latter is the "mem-

brana ovuli externa" of BERNHARDT, and its transparent substance is the "spatium pellucidum" of this author. g^1. Ruptured tunica granulosa, the "zona granulosa" of BERNHARDT (Symbolæ, &c., fig. xxiii. pp. ab. ab.).

Fig. 73. Ox (*Bos Taurus*, LINN.). *g*. Some of the peculiar granules of the ovisac with their nucleus. In one instance a single granule presents two nuclei (8. 10.). This granule measured $\frac{1}{1\,7\,0}$th Paris line in length. 800 *diam.*

Fig. 74. Hog (*Sus Scrofa*, LINN.). A patch of parasitic ovisacs (*h*) (57.) in (*h i*) the parietes of a Graafian vesicle. There is seen in several a tapering at one end (3.). They are all more spherical than when first formed (3.). *f*. The ovum. 60 *diam.*

Fig. 75. Dog (*Canis familiaris*, LINN.). Part of a Graafian vesicle (*h i*), with parasitic ovisacs (57.) (*h*) in its parietes. Compare with fig. 76. from the Pigeon. 100 *diam.*

Fig. 76. Pigeon (*Columba livia*, LINN.). Ovisac with its vascular covering; the union of these two presenting (*h i*) a structure analogous to the Graafian vesicle of Mammals (47.). In the parietes of this structure, and probably *included* (57.) on the acquisition by the ovisac of its covering, are several parasitic ovisacs (*h*). The vesicle has been crushed, but the minute parasitic ovisacs remain unmoved. The yelk has escaped. *g*. Peculiar granules passing slowly out of the vesicle. Compare these with the corresponding granules in Mammalia (8—10.); and compare the parasitic ovisacs in this figure with those in fig. 75. from the Dog. 60 *diam.*

Fig. 77. Ox, Heifer (*Bos Taurus*, LINN.). Ovary, with the outer third of a Graafian vesicle reflected. In the parietes of the Graafian vesicle there is seen transversely divided (*h*) a parasitic ovisac (58.), or incipient Graafian vesicle. *Actual size.*

Fig. 78. Ditto. Enlarged view of the two halves of the divided parasitic ovisac, or incipient Graafian vesicle, seen in fig. 77. *f*. Ovum; g^3. membrana granulosa; *h*. proper membrane of the parasitic ovisac.

Fig. 79. Ditto. Transverse section of the proper membrane (*h*) of the parasitic ovisac, or incipient Graafian vesicle, shown in figs. 77. and 78, here removed from its proper cavity (6.). The membrana granulosa (g^3) has fallen down. 25 *diam.*

Fig. 80. (from BAER). "Ovule avec le disque proligère (g^2) d'une vache, grossie dix fois." Contrast with fig. 55. Plate VI.

XVI. *Magnetical Observations made in the West Indies, on the North Coast of Brazil and North America, in the Years* 1834, 1835, 1836, *and* 1837, *by Captain Sir* Everard Home, *Bart. R.N. F.R.S. Reduced by the Rev.* George Fisher, *M.A. F.R.S.*

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It was the object in the experiments recorded in this paper, to determine the relative magnetic forces soliciting both the dipping, and horizontal needles, by observing the times of their completing a given number of vibrations at the various places visited during a period of three years, on the North American and West India Station, in Her Majesty's Ship Racehorse.

The dipping instrument used was one of modern construction by Dollond. Each observation for the dip consisted of an equal number of readings of the positions of the needle, with the face of the instrument east and west, before and after the inversion of the poles, and a mean of all the readings taken for the true dip. The instrument had two needles fitted to it, one of which being used solely for the purpose of observing its vibrations, its magnetism was therefore never interfered with, and this needle in this paper is distinguished by the letter B. The other needle was kept for the purpose of determining the dip, and the results obtained with it are given in Table I.

For the purpose of determining the horizontal forces, besides the needle B, four other needles, Nos. 1, 2, 3, and 4, were used, the first two, four and a half, the others, three inches in length, and of a cylindrical form. When used they were suspended by fibres of raw silk in an apparatus constructed for the purpose. They had fixed centres of suspension, with sliding weights for their horizontal adjustments. The vibrations were confined to the same limits of arc, so as to render the experiments as comparative as possible, and to avoid any correction for circular arc. The state of the thermometer was also registered, though at present it is impossible to assign with any degree of accuracy the correction due to the results arising from the differences of temperatures at which the experiments were made.

The needles Nos. 1, 2, 3, and 4 appear, by comparing the experiments made near London previous to the voyage, with similar ones at the same place at the expiration of it, to have lost during the interval some portion of their magnetism; the same circumstance is also observable from the experiments made at Bermuda and other places. To compensate for this change in the intensities of the needles, the experiments contained within the horizontal lines in the Tables in which they are recorded

have been reduced to the same epoch, or mean period of observation, by the application of a proportional part of this change; and the corrected or concluded times of completing a given number of vibrations for each needle, together with the corresponding forces, which are inversely as the squares of these times, are given in separate columns. These forces are again reduced, and compared with the horizontal force at Plymouth, which is taken equal to unity.

The magnetism of the needle B appears to have been subject to some occasional changes during the voyage, but not of sufficient magnitude or regularity as to admit of any correction being made on this account. The mean of all the experiments collectively, that were made at the same place with this needle, is therefore taken as one result, and compared with others obtained in a similar way at the other places.

Some degree of uncertainty is attached to experiments of this nature when made with various needles, the whole of which have only been occasionally used, and the magnetism of these subject to various changes, on account of the difficulty of assigning such values for the final results as the experiments themselves seem best to justify, since the means as deduced from the experiments at each place are on this account somewhat vitiated, being no longer *strictly* comparative; and moreover the same experiments may by different persons be differently compared with each other, and thereby results somewhat different may be obtained.

It is necessary to state, therefore, that the horizontal forces at Plymouth, Bermuda, and Port Royal in Jamaica have been determined by the experiments made at Plymouth in March 1834, immediately before leaving England, compared with similar ones made at the two latter places in the months of June and July following, and by applying a small correction for the diminution in the intensities of the respective needles. The experiments at Chagres are compared with the previous and subsequent ones at Jamaica made at short intervals of times; those at Parà, Maranham, and Demerara with those made at Bermuda; and lastly those at Antigua with the previous and subsequent ones at Bermuda. The experiments thus grouped and compared together are contained between the horizontal lines in the Tables II., III., IV., and V., together with the comparative horizontal forces between these places; these forces are then compared with the force at Plymouth, and the results given in a separate column.

Having thus obtained the horizontal forces at each place as compared with Plymouth, these are again reduced to the forces in the direction of the dipping needle by multiplying the horizontal forces by the fraction $\frac{\text{cos. dip at Plymouth}}{\text{cos. dip at Place}}$. The comparative whole forces being deduced by means of the horizontal needles, and also by direct experiment with needle B when vibrated in the dipping instrument, the final results are given in Table VIII.

G. F.

Table I.

Containing the Dip as observed at different places.

Dates.	Places.	Lat.	Long.	Dip.	Remarks.
1833. July 20 and 29.	Near London ..	51° 26′ N.	0° 18′ W.	69° 52′ 38″	Observations made at Ham, near London.
1834. Jan. 4.	,,	,,	,,	70 17 42	Observations made at the same place.
Mar. 3.	Plymouth.....	50 22	4 9	69 58 2	At the Athenæum.
11.	,,	,,	,,	69 56 20	,,
April 2.	,,	50 20	4 7	69 44 54	On the Breakwater (in the centre).
9.	,,	,,	,,	69 41 10	,,
May 27.	Halifax.....	44 39	63 37	75 33 1	At the Observatory in the Dockyard.
June 11.	Bermuda.....	32 18	64 50	67 46 21	In the Dockyard.
16.	,,	,,	,,	67 44 12	,,
July 11.	Jamaica	17 55	76 50	47 0 42	On the Point at Port Royal.
11.	,,	,,	,,	47 5 7	,,
Aug. 1.	,,	,,	,,	47 4 41	,,
Sept. 11.	Nicaragua	10 58	83 43	34 2 54	Point Arenas.
12.	,,	,,	,,	34 7 42	
18.	Chagres	9 20	80 1	32 36 4	Castle of St. Lorenzo (on the platform).
18.	,,	,,	,,	32 31 5	
19.	,,	,,	,,	32 23 26	
30.	Jamaica	17 55	76 50	46 49 16	Port Royal Point.
Oct. 6.	,,	,,	,,	47 6 40	
1835. Feb. 6.	Alta Vela.....	17 28	71 39	47 38 32	
April 30.	Antigua	17 3	61 50	48 46 15	At the Fort in English Harbour.
May 11.	Barbadoes	13 5	59 37	43 45 56	Engineers' Wharf, Shot-hall, Carlisle Bay.
June 5.	Parà.....	1 26 S.	48 30	23 59 35	At the Consul's house (in the garden).
July 14.	,,	,,	,,	24 20 50	
26.	,,	,,	,,	23 56 52	
27.	,,	,,	,,	23 56 8	
Oct. 1.	Maranham	2 30	44 18	23 23 3	On Point Francisco.
5.	,,	,,	,,	23 30 37	,,
16.	,,	,,	,,	23 29 54	,,
16.	,,	,,	,,	23 39 41	,,
1836. Jan. 5.	Barbadoes	13 5	59 37	43 28 45	The same place as before.
Feb. 6.	Bermuda.....	32 18	64 50	67 17 42	,,
April 11.	City of Caraccas	,,	,,	37 16 25	
May 30.	Demerara.....	,,	,,	33 56 27	In the Governor's garden.
July 26.	Parà.....	1 26	48 30	23 31 40	
26.	,,	,,	,,	23 19 0	
1837. June 7.	Halifax.....	44 39	63 37	74 57 52	In the Dockyard (at the observatory).
29.	Plymouth.....	50 20	4 7	69 38 32	Breakwater (in the centre).
Oct. 6.	Near London ..	51 26	0 18	69 25 12	At Ham, near London.

Note.—The dips and vibrations were for the greater part repeated by Mr. Byron Drury, Midshipman of the Racehorse, at the same times that the above observations were made.

Table II.

Experiments with Needle No. I.

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 300 horizontal vibrations.	Concluded Times of 300 vibrations at the places between the horizontal lines, when reduced to the same epoch.	Comparative horizontal intensities at the places between the lines.	Comparative horizontal intensities. The force at Plymouth being unity.
		h m	°	s	s		
Plymouth	1834. Mar. 17.	2 0 P.M.	60·2	1284·76	1284·76	1·000	1·000
Bermuda	June 17.	2 30	88·0	1113·70	1108·50	1·343	1·343
Jamaica ..	July 19.	Noon	90·0	917·50	911·84	1·985	1·985
Jamaica ..	July 19.	Noon	90·0	917·50			
Chagres ..	Sept. 19.	1 46 P.M.	87·5	891·45	891·45	1·064	2·113
Jamaica ..	Oct. 3.	2 0	89·5	920·17	919·67	1·000	
Parà	1835. July 20.	2 0 P.M.	88·6	1009·81	1010·89	1·409	1·892
Maranham	Oct. 3.	3 0	91·5	1017·70	1017·10	1·391	1·869
Maranham	Nov. 9.	2 10	89·0	1015·83			
Bermuda ..	1836. Feb. 7.	1 6	75·0	1199·83	1199·83	1·000	
Demerara	May 31.	1 49	85·0	997·75	997·40	1·447	1·943
Parà	Oct. 6.	4 8	94·0	1011·98			
Maranham	Nov. 18.	11 30 A.M.	92·0	1017·77			
Bermuda ..	1834. June 17.	2 30 P.M.	88·0	1113·70			
Antigua ..	1835. April 30.	3 34	88·5	968·81	968·81	1·433	1·925
Bermuda ..	1836. Feb. 7.	1 6	75·0	1199·83	1159·40	1·000	

Table III.

Experiments with Needle No. II.

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 300 horizontal vibrations.	Concluded Times of 300 vibrations at the places between the horizontal lines, when reduced to the same epoch.	Comparative horizontal intensities at the places between the lines.	Comparative horizontal intensities. The force at Plymouth being unity.
		h m	°	s	s		
Plymouth	1834. Mar. 26.	2 30 P.M.	59·4	1190·06	1190·06	1·000	1·000
Bermuda ..	June 17.	3 6	88·0	1033·60	1032·45	1·329	1·329
Jamaica ..	July 19.	1 0	90·5	853·43	852·12	1·950	1·950
Jamaica ..	July 19.	1 0	90·5	853·43			
Chagres ..	Sept. 19.	2 10	100·0	834·66	834·66	1·056	2·059
Jamaica ..	Oct. 3.	2 45	87·5	858·71	857·70	1·000	
Parà	1835. July 20.	3 0	90·8	881·93	884·16	1·405	1·866
Maranham	Oct. 3.	4 5	88·0	888·06	889·65	1·387	1·843
Maranham	Nov. 9.	3 25	88·0	887·85			
Bermuda ..	1836. Feb. 7.	2 5	76·5	1047·53	1047·91	1·000	
Demerara	May 31.	2 22	83·0	872·50	871·38	1·446	1·922
Parà	Oct. 6.	5 0	86·5	886·40			
Maranham	Nov. 18.	1 0	95·0	893·04			
Bermuda ..	1834. June 17.	3 6	88·0	1033·60	1040·98	1·000	
Antigua ..	1835. April 30.	4 14	84·0	893·47	893·47	1·357	1·804
Bermuda ..	1836. Feb. 7.	2 5	76·5	1047·53			

Table IV.

Experiments with Needle No. III.

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 300 horizontal vibrations.	Concluded Times of 300 vibrations at the places between the horizontal lines when reduced to the same epoch.	Comparative horizontal intensities at the places between the lines.	Comparative horizontal intensities. The force at Plymouth being unity.
		h m	°	s	s		
Plymouth	1834. Mar. 24.	3 0 P.M.	58·0	953·40	953·40	1·000	1·000
Bermuda..	June 17.	3 40	88·0	832·80	832·07	1·313	1·313
Jamaica ..	July 19.	0 43	91·0	691·30	690·49	1·907	1·907
Jamaica ..	July 19.	0 43	91·0	691·30			
Chagres ..	Sept. 19.	2 38	99·0	670·58	670·58	1·067	2·035
Jamaica ..	Oct. 3.	4 0	93·0	693·20	692·84	1·000	
Parà	1835. July 23.	4 0	88·5	707·89	710·30	1·408	1·849
Maranham	Oct. 3.	4 38	86·5	712·25	711·92	1·402	1·840
Maranham	Nov. 9.	4 34	85·0	711·98			
Bermuda..	1836. Feb. 7.	2 47	72·5	842·73	842·91		
Demerara	May 31.	3 0	83·0	701·22	700·57	1·447	1·901
Parà	Oct. 6.	5 34	84·0	712·71			
Maranham	Nov. 19.	1 34	94·5	711·53			
Bermuda..	1834. July 17.	3 40	88·0	832·80	838·07	1·000	
Antigua ..	1835. Apr. 30.	5 32	79·5	665·60	665·60	1·585	2·081
Bermuda..	1836. Feb. 7.	2 47	72·5	842·73			

Table V.

Experiments with Needle No. IV.

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 300 horizontal vibrations.	Concluded Times of 300 vibrations at the places between the horizontal lines when reduced to the same epoch.	Comparative horizontal intensities at the places between the lines.	Comparative horizontal intensities. The force at Plymouth being unity.
		h m	°	s	s		
Plymouth	1834. Mar. 24.	4 50 P.M.	56·9	874·55	874·55	1·000	1·000
Bermuda..	June 17.	4 20	89·0	759·98	757·90	1·331	1·331
Jamaica ..	July 19.	3 55	87·5	626·93	624·75	1·959	1·959
Jamaica ..	July 19.	3 55	87·5	626·93			
Chagres ..	Sept. 19.	3 36	95·0	610·08	610·08	1·075	2·107
Jamaica ..	Oct. 3.	5 15	85·7	633·92	632·59	1·000	
Parà	1835. July 23.	4 0	86·9	654·06	656·02	1·413	1·882
Maranham	Oct. 3.	5 2	83·5	659·65	660·65	1·394	1·856
,,	Nov. 9.	5 13	82·5	659·73			
Bermuda..	1836. Feb. 7.	3 17	72·0	779·53	779·95	1·000	
Demerara	May 31.	3 45	82·5	649·08	647·33	1·452	1·933
Parà	Oct. 6.	6 4	84·0	657·98			
Maranham	Nov. 19.	2 47	90·0	661·95			

Table VI.

Experiments with Needle B. [Horizontal Vibrations.]

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 200 horizontal vibrations.	Concluded Times of 200 horizontal vibrations.	Comparative horizontal intensities.
		h m	°	s	s	
Plymouth ..	1834. Mar. 26.	3 0 P.M.	59·7	915·67	915·67	1·000
Halifax	May 27.	Noon	50·7	977·95	973·09	0·885
Halifax	1837. June 7.	3 42 P.M.	64·0	968·23		
Bermuda....	1834. June 15.	2 20	88·2	794·98	801·90	1·304
Bermuda....	1836. Feb. 7.	2 0	84·0	808·82		
Jamaica	1834. July 16.	3 30	87·7	656·47	659·04	1·930
Jamaica	Oct. 1.	1 15	90·0	661·62		
Nicaragua ..	Sept. 11.	1 39	84·0	635·43	635·43	2·076
Chagres	Sept. 18.	Noon	100·0	639·80	639·68	2·049
Chagres	Sept. 19.	11 A.M.	84·0	639·56		
Alta Vela ..	1835. Mar. 6.	0 25 P.M.	92·0	686·40	686·40	1·780
Antigua	April 30.	3 30	89·2	688·33	688·33	1·770
Barbadoes ..	May 11.	2 0	86·5	677·51	677·51	1·827
Parà	July 23.	1 0	87·8	677·66	676·59	1·832
Parà	1836. Oct. 6.	5 0	100·0	675·57		
Maranham ..	1835. Oct. 21.	4 0	91·5	687·18	690·76	1·757
Maranham ..	1836. Nov. 18.	10 30 A.M.	93·0	694·34		
Demerara ..	May 31.	1 10 P.M.	85·0	674·00	674·00	1·846

Table VII.

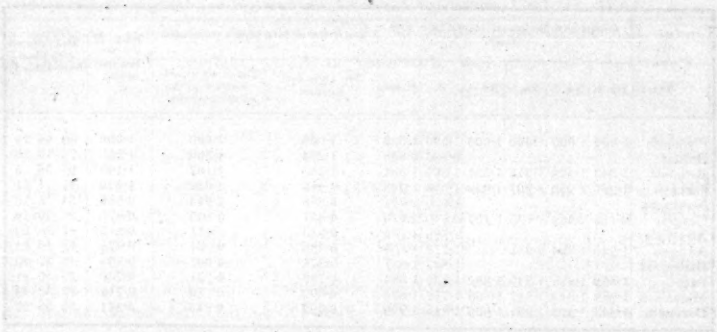
Experiments with Needle B. [In the direction of the Dipping Needle.]

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 40 vibrations in the direction of the dipping needle.	Concluded Times of 40 vibrations as before.	Comparative intensities in the direction of the dipping needle.
		h m	°	s	s	
Plymouth ..	1834. Mar. 24.	3 0 P.M.	56·0	112·52	111·49	1·000
Plymouth ..	1837. June 29.	3 0	84·0	110·45		
Halifax	1834. May 27.	3 0	52·0	98·05	97·60	1·306
Halifax	1837. June 7.	Noon	64·0	97·15		
Bermuda....	1834. June 13.	3 30 P.M.	93·5	100·49	102·34	1·187
Bermuda....	1836. Feb. 6.	3 30	72·0	104·20		
Jamaica	1834. July 11.	4 30	99·0	108·50	108·72	1·052
Jamaica	Oct. 6.	11 0 A.M.	86·0	108·95		
Nicaragua ..	Sept. 11.	2 0 P.M.	78·0	114·75	114·75	0·944
Chagres	Sept. 18.	Noon	92·0	117·57	117·57	0·899
Alta Vela ..	1835. Feb. 6.	Noon	113·0	116·80	116·80	0·911
Antigua	April 30.	3 0 P.M.	94·0	113·70	113·70	0·961
Barbadoes ..	May 11.	Noon	87·0	115·05	117·08	0·907
Barbadoes ..	1836. Jan. 5.	3 0 P.M.	85·0	119·10		
Demerara ..	May 30.	Noon	86·0	121·25	121·25	0·845
Parà	1835. July 9.	1 40 P.M.	95·0	126·97	126·97	0·771
Maranham ..	Oct. 7.	4 0	90·4	130·27	130·27	0·732

Table VIII.

Abstract of the Experiments.

Places.	Comparative horizontal intensities by each needle.						Comparative intensities in the direction of the dipping needle as deduced from		Means of the two last columns.	Mean dip at each place from Table I.
	No. 1.	No. 2.	No. 3.	No. 4.	B.	Means.	The horizontal needles.	The direct experiments with the dipping needle.		
Plymouth	1·000	1·000	1·000	1·000	1·000	1·000	1·000	1·000	1·000	69° 44′ 19″
Halifax	....	....	....	....	0·885	0·885	1·204	1·306	1·255	75 15 26
Bermuda	1·343	1·329	1·313	1·331	1·304	1·324	1·203	1·187	1·195	67 36 5
Jamaica	1·985	1·950	1·907	1·959	1·930	1·946	0·988	1·052	1·020	47 1 15
Nicaragua	....	....	....	....	2·076	2·076	0·868	0·944	0·906	34 5 18
Chagres	2·113	2·059	2·035	2·107	2·049	2·073	0·851	0·899	0·875	32 30 10
Alta Vela	....	....	....	....	1·780	1·780	0·915	0·911	0·913	47 38 32
Antigua	1·925	1·804	2·081	....	1·770	1·895	0·996	0·961	0·978	48 46 15
Barbadoes	....	....	....	....	1·827	1·827	0·874	0·907	0·890	43 37 20
Parà	1·892	1·866	1·849	1·882	1·832	1·864	0·706	0·771	0·738	23 50 41
Maranham	1·869	1·843	1·841	1·856	1·757	1·833	0·692	0·732	0·712	23 30 49
Demerara	1·943	1·922	1·901	1·933	1·846	1·909	0·797	0·845	0·821	33 56 27



XVII. *On the Geometrical Forms of Turbinated and Discoid Shells.*

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THE surface of any turbinated or discoid shell may be imagined to be generated by the revolution about a fixed axis (the axis of the shell) of the perimeter of a geometrical figure, which, remaining always geometrically similar to itself, increases continually its dimensions.

In discoid shells the generating figure retains its position upon the axis as it thus revolves, as in the *Nautilus Pompilius* (Plate IX. fig. 3.), and the *Argonaut*. In turbinated shells, including the great families of Trochi, Turbines*, Murices and Strombi, it slides continually along the axis of its revolution (fig. 4.). In some great classes of shells, as the Ammonites, the *Nautilus scrobiculatus*, the *Nautilus spirula*, the *Helix cornea*, the *Trochus perspectivus*, the Nerita, the generating figure increases its distance from the axis at the same time that it increases its dimensions and revolves.

Among the generating figures of conchoidal surfaces are to be found various known geometrical forms. The generating figure of the *Conus Virgo* is a triangle, that of the *Trochus telescopicus* and of the *Trochus Archimedis*, a trapezoid. The species of the genus Turbo have for their generating figure a curve, of double curvature, of a circular or elliptic form, to whose perimeter the axis of revolution is a tangent. The *Nautilus Pompilius* is generated by the revolution about its shorter diameter of a plane curve, approaching very nearly to a semi-ellipse (fig. 3.); and the Cypræa by the revolution of a similar curve about its longer diameter.

There is a mechanical uniformity observable in the description of shells of the same species, which at once suggests the probability that the generating figure of each increases, and that the spiral chamber of each expands itself, according to some simple geometrical law common to all. To the determination of this law, if any such exist, the operculum lends itself, in certain classes of shells, with remarkable facility. Continually enlarged by the animal, as the construction of its shell advances, so as to fill up its mouth, the operculum measures the progressive widening of the spiral chamber, by the progressive stages of its growth.

Of these progressive stages of the growth of the operculum, distinct traces remain

* The beautiful shell *Turbo scalaris* (Ventletrap) may be taken as an easy illustration of the properties to be described in this paper.

on its surface, under the form in the Turbines (fig. 1.) of certain *curved* lines, and in the Neritæ (fig. 2.) of certain *straight* lines, passing from the margin of the operculum (which if produced they would intersect) to a certain spiral line marked deeply upon its face. To this spiral they are tangents, and may be supposed to generate it by their consecutive intersections. The spiral eventually passes into the margin of the operculum, and for a considerable distance traces it.

If the eye be made to traverse one of the curved lines first spoken of in the operculum of the Turbo, or one of the straight lines in the Nerita, from its margin to the point where it loses itself in the spiral, and if it then follow the spiral until it returns to the point in the margin whence it set out, it will have traversed the boundary of a figure which was once the actual boundary of the operculum, which therefore indicates one stage of its growth, and of which all, similarly traced, will be seen to have similar geometrical forms.

It will further be apparent from this examination, that the operculum has increased at each stage of its growth, not throughout its whole margin at once, but on a series of different portions of it lying in different consecutive positions round it; each such addition being so made as to preserve the above-mentioned geometrical similarity of the whole*. In all the similar geometrical figures thus visible upon the face of the operculum, and which have in succession constituted its limits, the pole of the spiral will moreover be seen to occupy a similar position. The linear dimensions of any two of them (P_1 C Q_1 and P_2 C Q_2) are then to one another as the radii vectores drawn to similar points in them, and therefore as those (P P_1 and P P_2) drawn to the extremities of the boundary by which they unite.

To determine, therefore, the law according to which the linear increase of the operculum takes place, that is, the law according to which the linear increase of the section of the chamber of the shell takes place, we have only to determine the law according to which the radii vectores, drawn to successive points of the spiral visible upon the operculum, increase, that is, we have only, geometrically, to determine the spiral.

Now in every case this spiral is the logarithmic spiral.

A slight inspection of it is sufficient to suggest the probability that the angle at which it intersects its radius vector is everywhere the same, and this supposition is fully confirmed by direct admeasurements grounded upon the following property of the logarithmic spiral, "That the distances of successive spires, measured upon the same radius vector produced, from the pole and from one another, are respectively in geometrical progression; the common ratio of the progression being in both cases $\epsilon^{2\pi \cot A}$, where A is the constant angle of the spiral†."

* The whole class of shells Haliotis affects the method of formation here described. The *shell* itself is in this class generated by additions upon one margin, as in other classes the *operculum* is generated.

† Let R_n, R_{n+1}, R_{n+2} be consecutive radii vectores taken as above, and R_0 the radius vector corresponding to $\Theta = 0$ $\therefore$ $R_n = R_0\, \epsilon^{\Theta \cot A}$, $R_{n+1} = R_0\, \epsilon^{(\Theta + 2\pi) \cot A}$, $R_{n+2} = R_0\, \epsilon^{(\Theta + 4\pi) \cot A}$ $\therefore$ $R_{n+1} = \epsilon^{2\pi \cot A}$. R_n and $R_{n+2} - R_{n+1} = \epsilon^{2\pi \cot A} (R_{n+1} - R_n)$.

The following distances were measured upon three different opercula from the poles of their spiral curves to their successive whorls; the distances in the same column being measured on the same radius vector produced. It will be perceived that for the same operculum these distances have the same ratio consecutively to one another; the deviation from this law in no case exceeding that error which of necessity attaches to the method of admeasurement.

Operculum of the Order Turbo, No. I.

Distance in inches.	Ratio.	Distance in inches.	Ratio.	Distance in inches.	Ratio.	Distance in inches.	Ratio.
·24	2·28	·16	2·31	·2	2·3	·18	2·3
·55	2·32	·37	2·3	·6	2·3	·42	2·24
1·28		·85		1·38		·94	

Operculum, No. II.

Distance in inches.	Ratio.	Distance in inches.	Ratio.	Distance in inches.	Ratio.
·32	3·9	·25	4	·155	3·87
1·25		1·04		·62	

Operculum, No. III.

Distance in inches.	Ratio.	Distance in inches.	Ratio.
·6	1·33	·74	1·28
·8	1·23	·95	1·28
·99	1·28	1·215	
1·27			

The spiral of the operculum is then a logarithmic spiral. Now its linear dimensions in the different successive stages of its progress have been shown to be as the successive radii vectores of its spiral. The increments of its linear dimensions are then as the increments of these radii vectores. But by a fundamental property of the logarithmic spiral, the increments of its radii vectores, corresponding to equal increments in their angles of revolution, are as the radii vectores themselves. Thus, then, it follows that the increments of the linear dimensions of the operculum, corresponding to equal angular distances round its pole, are as its existing linear dimensions; and, therefore, that the increments of the linear dimensions of the section of the spiral chamber corresponding to these are everywhere as its existing linear dimensions.

The animal, as he advances in the construction of his shell, increases continually his operculum, so as to adjust it to its mouth.

He increases it, however, not by additions made at the same time all round its

margin, but by additions made only on one side of it at once. One edge of the operculum thus remains unaltered as it is advanced into each new position, and placed in a newly formed section of the chamber similar to the last, but greater than it.

That the same edge which fitted a portion of the first less section should be capable of adjustment, so as to fit a portion of the next similar but greater section, supposes a geometrical provision in the curved form of the chamber of great apparent complication and difficulty. But God hath bestowed upon this humble architect the practical skill of a learned geometrician, and he makes this provision with admirable precision in that curvature of the logarithmic spiral which he gives to the section of the shell. This curvature obtaining, he has only to turn his operculum slightly round in its own plane as he advances it into each newly formed portion of his chamber, to adapt one margin of it to a new and larger surface and a different curvature, leaving the space to be filled up by increasing the operculum wholly on the other margin.

To make this apparent, let the following be received as a characteristic property of the logarithmic spiral: "That lines anywhere drawn from its pole, inclined to one another at the same angle, will intercept between them branches of the curve which, however different their linear dimensions, will be geometrically similar to each other." So that if two lines given in position be imagined to be drawn from the pole of such a spiral, parallel to its plane, and the spiral be then imagined to be put in motion in its own plane round its pole, then as its curve revolved under these lines they would intercept portions of it continually increasing, or continually diminishing in dimensions, and continually receding from or approaching the pole, but all geometrically similarly to each other and similarly placed."

Now each new section of the chamber of the shell being similar to the preceding section, but greater than it, if the operculum were thrust forward into this greater section without being turned round in its own plane, any portion of its edge would manifestly present to the corresponding portion of the perimeter of the new section a similar but a less curve, which could not be made to coincide with it. If, however, the operculum be imagined to be turned round in its own plane about its pole in the opposite direction to that in which the spiral increases, the curve presented by it to this portion of the perimeter of the section will continually approach it, increasing its dimensions, but remaining similar to it, so that at length it will coincide with it. And thus one margin of the operculum will be made everywhere to fit itself to the side of the chamber, the coincidence of the other margin remaining to be produced by new matter added to it.

It will be apparent from a simple inspection of the operculum that the animal does thus turn it round in its own plane as he advances it, with what is called a screw motion.

Such is the theory of the growth of the operculum. There is traced in it the application of properties of a geometric curve to a mechanical purpose by HIM who metes the dimensions of space and stretches out the forms of matter according to the

rules of a perfect geometry,—properties which, like so many others in nature, may have also their application in art. It instructs us how to shape a tube of a variable section, so that a piston driven along it shall, by one side of its margin, coincide continually with its surface as it advances, provided only the piston be made at the same time continually to revolve in its own plane.

The investigation has now arrived at a point from which the law of the geometrical description of turbinated shells can be enunciated with greater precision. "They are generated by the revolution about a fixed axis (the axis of the shell) of a curve, which continually varies its dimensions according to the law, that each linear increment, corresponding to a given angular increment, shall vary as the existing dimensions of the line of which it is the increment (the law of the description of the logarithmic spiral), and which curve either retains its position upon the axis, or moves along it with a motion of translation in the direction of its length."

This law is readily subjected to verification by admeasurement.

It is clear that, if it obtain, similar linear dimensions measured at similar points of successive whorls, should be in geometric progression. Thus if the generating curve (as in the *Nautilus Pompilius*) revolve about the axis without at the same time sliding along it, and a section be made through the centre of the shell perpendicular to the axis, then will the section be (if this law be true) a spiral curve, whose distances from the axis, measured on the same radius vector, are in geometrical progression, and which is therefore a logarithmic spiral.

In the more general case, in which the generating curve, as in the *Turbo scalaris*, slides forwards upon the axis as it revolves, increasing at the same time its linear dimensions according to the law of the logarithmic spiral, it is clear that the surfaces of the successive whorls would interfere with one another, and that thus the uniformity of the spiral chamber would be destroyed, unless the motion of translation (or the sliding motion) of the curve, by which the space allowed to each whorl upon the axis is determined, were governed by some law corresponding to that which governs the linear dimensions of the whorl; unless, in short, the spaces allowed to the widths of successive whorls upon the axis varied in the same progression as the widths themselves vary. A similar principle applies to the distances of the whorls measured upon the surface of the shell in the same plane passing through the axis. These distances are, in fact, in this case, similar linear dimensions of successive whorls, and are therefore subject, according to the theory, to the law of the logarithmic spiral, and like the distances of successive whorls of that spiral, on the same radius vector, are in geometric progression.

Nautilus Pompilius.

These conclusions were directly verified by the following observations. A shell of the *Nautilus Pompilius* was cut through the middle in a direction perpendicular to its axis, and a tracing was taken of the section of its spiral surface; this tracing is copied in fig. 6.

It was made from the dark line which shows, on the section of the internal whorls of the shell, the line of that pearly surface which the animal deposits as a covering to its completed portion, as it advances in the construction of it. It is important to make this observation, because as it extends one whorl of its shell over another, the animal deposits continually upon the pearly surface of this last a new coating of shell, and thickens it; and it is in the centre of this thickened section that is to be found that section of the pearly surface, of which the edge of the external whorl is a continuation, and from which this tracing was taken.

It will be found that the distance of any two of its whorls measured upon a radius vector is *one-third* that of the two next whorls measured upon the same radius vector. Thus

$a\,b$ is one-third of $b\,c$,
$d\,e$ is one-third of $e\,f$,
$g\,h$ is one-third of $h\,i$,
$l\,k$ is one-third of $l\,m$.

The curve is therefore a *logarithmic spiral.*

Turbo duplicatus.

From the apex of a large specimen of the *Turbo duplicatus* a line was drawn across its whorls, and their widths were measured upon it in succession, beginning from the last but one. The measurements were, as before, made with a fine pair of compasses and a diagonal scale. The sight was assisted by a magnifying glass.

In a parallel column to the admeasurements are the terms of a geometric progression, whose first term is the width of the widest whorl measured, and whose common ratio is 1·1804.

Widths of successive whorls measured in *inches* and parts of an *inch*.	Terms of a geometrical progression, whose first term is the width of the widest whorl, and whose common ratio is 1·1804.
1·31	1·31
1·12	1·1098
·94	·94018
·8	·79651
·67	·67476
·57	·57164
·48	·48427
·41	·41026

Yet further to verify this remarkable coincidence of the widths of successive whorls with the mathematical law of a geometric progression, the following property of such a progression was determined: "that μ representing the ratio of the sum of every even number (m) of its terms to the sum of half that number of terms, the common ratio (r) of the series is represented by the formula

$$r = (\mu - 1)^{\frac{2}{m}}."$$

The following measurements were then made, beginning from the second and third whorls respectively:

Width of six whorls in inches.	Width of three whorls in inches.	Ratio μ.
5·37	2·03	2·645
4·55	1·72	2·645
Width of four whorls in inches.	**Width of two whorls in inches.**	**Ratio μ.**
4·15	1·74	2·385
3·52	1·47	2·394

By the two first admeasurements the formula gives

$$r = (1{\cdot}645)^{\frac{1}{3}} = 1{\cdot}1804.$$

By the mean of the ratios deduced from the two second admeasurements it gives

$$r = (1{\cdot}389)^{\frac{1}{3}} = 1{\cdot}1806.$$

It is scarcely possible to imagine a more accurate verification than is deduced from these larger admeasurements, and we may with safety annex to the species *Turbo duplicatus* the characteristic number 1·18.

Buccinum subulatum.

A line was drawn from the apex of this shell across its whorls as in the last, and the following admeasurements were similarly made.

Widths of successive whorls by admeasurement in inches.	Terms of a geometrical progression, whose first term is the width of the widest whorl and ratio, 1·18.
1·14	1·14
1·00	1·0089
·9	·89279
·79	·79008
·7	·69919
·62	·61875
·54	·54757

In verification of the above the following larger admeasurements were made, beginning respectively from the last whorl, the last but one, and the last but two.

Width of six whorls.	Width of three whorls.	Ratio μ.
5·10	2·08	2·45
4·52	1·84	2·45
3·94	1·60	2·45

From these admeasurements we have, by the formula $r = (\mu - 1)^{\frac{2}{m}}$,

$$r = (1{\cdot}45)^{\frac{1}{4}} = 1{\cdot}1318.$$

Turbo phasianus.

Three lines were drawn from the apex of this shell in different directions across its whorls, and the following admeasurements were made upon them:

Widths of successive whorls in inches by admeasurement.	Terms of a geometrical progression, whose first term is the widest whorl and ratio, 1·75.
First line.	First line.
2·55	2·55
·44	·44
·25	·251
Second line.	Second line.
·98	·98
·56	·56
·32	·323
Third line.	Third line.
·7	·7
·4	·4
·23	·228

The remarkable accordance of the measured with the calculated widths of the whorls in this shell is to be attributed to the precision with which the line of separation of the whorls is traced upon it. A great number of admeasurements were similarly made upon other shells of the genera Trochus, Strombus and Murex; some of them were cut through the axis longitudinally; and similar measurements were made by drawing lines from the apex across the section. From all these the same result was obtained.

Thus to each particular species of shell is annexed a characteristic number, being the ratio of the geometric progression of similar successive linear dimensions of its whorls; from which number is deducible the constant angle of the particular logarithmic spiral which is affected by that species of shell (see equation 26. in the following mathematical discussion). This number, or this angle, connected as it is of necessity with the circumstances of the animal's growth and the manner of its existence, is determinable by actual admeasurement, and may be available for the purposes of classification: it may suggest relations to which the observations of naturalists may usefully be directed, and eventually become linked with characteristic forms and modes of molluscous existence*.

* The whole race of land shells, for instance, will certainly (from the nature of the case) be distinguished from the aquatic shells by a wide difference in the numbers characteristic of the species of the two groups.

Why the Mollusks who inhabit turbinated and discoid shells should, in the progressive increase of their spiral dwellings, affect the particular law of the logarithmic spiral, is easily to be understood. Providence has subjected the instinct which shapes out each, to a rigid *uniformity* of operation.

This uniformity manifests itself in turbinated shells in respect to their axes. Now the law of the logarithmic spiral, considered under its more general form of a curve of double curvature, is the only one according to which the Mollusk can wind its spiral dwelling in an uniform direction through the space round its axis, in respect to that axis. Under this general form it may be geometrically defined as the curve whose tangent retains always the same angular position in respect to its axis*, and in respect to a line drawn from the point where it touches the curve perpendicular to the axis; or in other words, which traverses the space round the axis always in the same direction in respect to it.

A second property of the logarithmic spiral, equally referring itself to the uniformity of the animal's operations about the axis of its shell, is this; that it has everywhere the same geometrical curvature, and is the only curve except the circle† which possesses this property.

Certain physiological facts having reference to the growth of the Mollusk are deducible from the geometrical description of its shell. If it be a *land* shell, its capacity may be supposed (reasoning from that principle of economy which is an observable law in Nature) to be precisely sufficient for the reception of the animal who built it. If it be an *aquatic* shell, it serves the animal at once as a habitation and as a float; enabling it to vary its buoyancy according as it leaves a greater or a less portion of the narrower extremity of its chamber unoccupied, and thus to ascend or descend in the water, at will. Now that its buoyancy, and therefore the facility of thus varying its position, may remain the same at every period of its growth, it is necessary that the increment of the capacity of its float should bear a constant ratio to the corresponding increment of its body, a ratio which always assigns a greater amount to the increment of the capacity of the shell than to the corresponding increments of the animal's bulk. Thus the chamber of the *aquatic* shell is increased, not only, as is the land shell, so that it may contain the greater bulk of the Mollusk, but so that more and more of it may be left unoccupied. Now the capacity of the shell and the dimensions of the animal began together, and they increase thus in a constant ratio; the whole bulk of the animal bears therefore a constant ratio, of greater inequality, to the whole capacity of the shell, in *aquatic* shells: in *land* shells, it is probably equal to it.

Now let the generating curve of a shell be conceived to describe, as it revolves round its axis, a series of successive equal angles, represented each by $\Delta\,\Theta$. Corresponding to these equal increments of the angle of revolution of the generating

* So that moved parallel to itself until it intersected the axis, it would always intersect it at the same angle.

† The circle may, in fact, be considered a logarithmic spiral, the constant inclination of whose tangent to its radius vector is a right angle. Of all curves, this spiral, considered as thus including the circle, is the simplest.

curve, will be certain increments of the capacity of the shell; and it appears from the following mathematical investigation of the properties of conchoidal surfaces (see equation 19.), that the increments of the capacity of the shell, thus taken, will be in a constant ratio to the then existing whole capacities of the shell. The increment of the animal's bulk corresponding to each of these increments of the shell must then be in a constant ratio to its then existing bulk; that is, the animal's growth corresponding to a given increment, $\Delta\,\Theta$, in the angle described by the generating curve of its shell, is proportional always to its existing growth.

Let us now suppose that the physical living energies of the animal (those by which it grows), at any time, are proportional to its then existing growth; and therefore that its growth in any increment of time is proportional to its growth up to that time (a supposition which possesses an independent probability). From the conclusion before arrived at, and from this supposition, it follows that the growth of the animal corresponding to a given increment, $\Delta\,\Theta$, in the angle of revolution of the generating curve, and the growth corresponding to a given increment of time, are each proportional to the animal's whole then existing growth, and therefore to one another; and, since they begin together, that the whole angle, Θ, of revolution of the generating curve of the shell, is proportional to the whole corresponding time of the animal's growth, and therefore that the whole number of whorls, and parts of whorls, is proportional to its whole age: a conclusion which, like the supposition whence it is deduced, possesses an independent probability.

The separate probability of each of the two suppositions, "that the physical energies of the Mollusk, as developed in its growth in a given increment of time, are proportional to its whole then existing growth*, and that its age is always proportional to the whole angle which, in the construction of the shell, it has then described round its axis," is greatly increased by the necessary relation which is here shown to obtain between them; a relation, by reason of which, either supposition being made, the other becomes a conclusion.

The form of the Mollusk being supposed to remain geometrically similar to itself, the surface of its mantle, by which organ it deposits its shell, of necessity varies as the square of its linear dimensions, whilst the whole bulk of the animal varies as the cube of its linear dimensions. But, as its whole bulk, varies its active living and growing energy, and therefore the amount of the deposition of its shell in a given time; this last, then, varies as the cube of its linear dimensions; but the surface of the depositing organ (the mantle) varies only as the square of the same linear dimensions. Besides, then, the organic increase of the surface of the mantle, there must be an increased functional activity of all its organs, varying as its simple linear dimensions.

This increased functional activity of the surface of the depositing organ, varying

* May not this law of the growth of a Mollusk have its analogy in other forms of animal life, and perhaps in vegetable life?

simply as the linear dimensions of the animal or its shell, offers an analogy, and has perhaps a relation, to the increase of the section of the shell, according to the same law of its simple linear dimensions.

Subjoined to this paper is a mathematical discussion of the following geometrical and mechanical elements of a conchoidal surface: its VOLUME, the dimensions of its SURFACE, the CENTRE OF GRAVITY of its contained solid, the CENTRE OF GRAVITY of its surface.

These elements are determined (the law of the logarithmic spiral being supposed) by certain transcendental functions, having constant factors dependent for their amount upon the statical moments and the moments of inertia of the generating figures and of their areas.

The object proposed in the determination of these elements was their application to a discussion of the hydraulic theory of shells; yet further, if possible, to develope that wisdom of God which shaped them out and moulded them; and especially in reference to the particular value of the constant angle which the spiral of each species of shell affects,—a value connected by a necessary relation with the economy of the material of each, and with its stability, and the conditions of its buoyancy*.

The paper concludes with a discussion of the general EQUATIONS to a conchoidal surface in respect to systems of polar and of rectangular coordinates.

To determine the Volume of a Conchoidal Solid.

Suppose the generating curve to be a plane curve, and let it (first) retain its position upon the axis as it revolves, varying its dimensions.

Let P C and Q C (fig. 3.) be two of its positions, inclined at the angle $\Delta\,\Theta$, and including between them the elementary solid P C Q.

Imagine the plane P C to have revolved about A z through the angle $\Delta\,\Theta$ without altering its dimensions, the solid generated by it would then, by the theorem of GULDINUS, be represented by $M\,.\,\Delta\,\Theta$, where M represents the statical moment of the plane P C about the axis A z.

The elementary solid imagined to be in like manner generated by the revolution of Q C through the angle $\Delta\,\Theta$, will similarly be represented by $(M + \Delta\,M)\,\Delta\,\Theta$.

Now between these two imaginary solids is evidently the actual elementary solid P C Q. Calling then V the volume of the solid to be determined, we have

$$M\,\Delta\,\Theta < \Delta\,V < (M + \Delta\,M)\,\Delta\,\Theta.$$

Or, considering M and V as functions of Θ, and expanding by TAYLOR's theorem,

$$M\,\Delta\,\Theta < \frac{d\,V}{d\,\Theta}\,\Delta\,\Theta + \frac{d^2\,V}{d\,\Theta^2}\cdot\frac{(\Delta\,\Theta)^2}{1\,.\,2} + \&c. < M\,\Delta\,\Theta + \frac{d\,M}{d\,\Theta}\,(\Delta\,\Theta)^2 + \&c.$$

* As illustrative of this remark, it may here be mentioned that the shell of the *Nautilus Pompilius* has, hydrostatically, an *A*-statical surface. If placed with any portion of its surface upon the water, it will immediately turn over towards its smaller end, and rest only on its mouth. Those conversant with the theory of floating bodies will recognise in this an interesting property.

And this is true for all values of Θ:

$$\therefore \frac{dV}{d\Theta} = M,$$

and

$$V = \int M \, d\Theta. \quad \ldots\ldots\ldots\ldots \quad (1.)$$

If we imagine the plane C Q to slide along the axis A z (fig. 4.) without otherwise altering its position, the elementary solid included between it and P C will retain the same volume as it had before; for the two planes P C and Q C may be divided into the same number of similar elements, whose corresponding angles being joined, the solid element included between them will be divided into as many pyramidal frusta, the volume of each of which will remain unaltered by the supposed displacement of C Q, since each such frustum may be imagined to be made up of two pyramids, the base of each of which will remain the same after the displacement, and their bases and vertices between the same parallels. Thus, then, the volume determined by the above formula is that of the conchoidal solid under its most general form.

To determine the Area of a Conchoidal Surface.

Let U represent the whole area of the surface (fig. 3.), and Δ U the elementary area intercepted between the positions P C and Q C of the generating curve, supposed to revolve without otherwise altering its position on the axis.

Take Δ S to represent the element P Q of the curve described by the extremity P of the revolving axis P C of the generating curve.

Imagine the generating curve to describe, without altering its dimensions, an angle about the axis A z, such that the circular arc, described on this supposition by the point P, may equal P Q or Δ S. This angle will be represented by $\frac{\Delta S}{R}$.

The generating curve remaining always similar to itself, its statical moment about A z is a function of Θ or of R. Let it be represented by N, and considered a function of R. The elementary surface which the curve C P will generate, on the supposition just made, will then be represented by $\frac{\Delta S}{R} \cdot N$, according to the property of Guldinus.

A surface similarly generated by C Q will in like manner be represented by

$$\frac{\Delta S}{R + \Delta R} (N + \Delta N).$$

Now the dimensions of the actual element of the conchoidal surface lie between the dimensions of these two imaginary surfaces.

This will be seen if we conceive any number of planes passing through the axis C D, at right angles to A z, to intersect all three of the surfaces spoken of. The intercepted parts will be strips of the three surfaces, all of the same length, but of

breadths, of which those of the surface described by P C will be the least, and those described by Q C the greatest.

$$\therefore \frac{N}{R} \Delta S < \Delta U < \frac{N + \Delta N}{R + \Delta R} \cdot \Delta S.$$

Considering, therefore, S, U, and N as functions of R, expanding by TAYLOR's theorem and dividing by ΔR

$$\frac{N}{R} \cdot \frac{dS}{dR} + \frac{d^2S}{dR^2} \cdot \frac{N}{R} \cdot \frac{\Delta R}{1.2} + \&c. < \frac{dU}{dR} + \frac{d^2U}{dR^2} \cdot \frac{\Delta R}{1.2} + \&c. < \frac{N}{R} \cdot \frac{dS}{dR} + \left\{ \frac{dS}{dR} \left(\frac{1}{N} \frac{dN}{dR} - \frac{1}{R} \right) + \frac{1}{2} \frac{d^2S}{dR^2} \right\} \Delta R + \&c.$$

The second of these series having, for all values of ΔR, a value intermediate between the other two, and the first terms of these other two being equal; the first terms of the three series are equal.

$$\therefore \frac{dU}{dR} = \frac{N}{R} \cdot \frac{dS}{dR}$$

and

$$U = \int \frac{N}{R} \cdot \frac{dS}{dR} \cdot dR \quad \ldots\ldots\ldots\ldots \quad (2.)$$

which to adapt it for integration, (R being a function of Θ) may be put under the form

$$U = \int \frac{N}{R} \cdot \frac{dS}{dR} \cdot \frac{dR}{d\Theta} \cdot d\Theta.$$

The expression for the area of the surface thus determined, on the supposition that the generating curve does not alter its position in respect to the axis otherwise than by revolving round it, is the same with that of the surface which would be generated by a curve which, as it revolved about the axis, slided along it, a different form being assigned to the function N. For if we imagine a conchoidal surface of this general form (fig. 4.) to be intersected by planes, exceedingly near to one another, passing through its axis, and at the same time to be traversed, as the surfaces of turbinated shells usually are, by spiral lines parallel to the direction of the whorl, and which may be understood to mark the paths of given points in the generating curve*; then each element of the surface intercepted between two of the planes spoken of will, by these spiral lines, be divided into a series of oblique parallelograms, two adjacent sides (containing an acute angle) of each of which, may be considered as intersections with the conchoidal surface of two planes, which intersect one another in an ordinate of the generating curve; one of these planes is a tangent to one of the spiral lines spoken of, and the other is the plane of the generating curve itself. Now let us suppose the inclination of these planes to one another to be *constant*, as is always the case in shells, and let it be represented by A. Let moreover the inclination, to its ordinate,

* This demonstration will be best understood by referring to the actual surface of a turbinated shell on which the spiral lines are visible.

of the tangent to the generating curve be represented by φ; and the inclination to the same ordinate of the tangent to the spiral line by σ. We have then given the inclination A of two planes to one another, and the inclinations φ and σ of two lines, drawn in them respectively, to the intersection of these planes; whence by a well-known formula of spherical trigonometry, if ι represent the inclination of these lines to one another,

$$\cos \iota = \cos \varphi \cos \sigma + \sin \varphi \sin \sigma \cos A.$$

Moreover, if the two adjacent sides of the parallelogram, being elements of the generating curve, and the spiral, be represented by Δs and ΔS; then since their inclination is ι, the area of the parallelogram is represented by $\Delta S \,.\, \Delta s \sin \iota$. Now let us suppose the generating curve to revolve, not altering its dimensions, but sliding along the axis; then

$$\sigma = \tfrac{\pi}{2} \therefore \cos \iota = \sin \varphi \cos A, \text{ and } \sin \iota = \sqrt{1 + \sin^2 A \tan^2 \varphi} \,.\, \cos \varphi;$$

also in this case

$$\Delta S = y \,\Delta \Theta \operatorname{cosec} A;$$

the area of the elementary parallelogram becomes then

$$y \sqrt{\operatorname{cosec}^2 A + \tan^2 \varphi} \,.\, \cos \varphi \,.\, \Delta s \,.\, \Delta \Theta, \text{ or } y \sqrt{\operatorname{cosec}^2 A + \frac{dx^2}{dy^2} \cdot \frac{dy}{ds}} \,\Delta s \,\Delta \Theta;$$

so that the whole surface of the elementary slice intercepted between two planes passing through the axis which are inclined to one another at an angle $\Delta \Theta$, is on this supposition,

$$\Delta \Theta \int y \sqrt{\operatorname{cosec}^2 A + \frac{dx^2}{dy^2}} \cdot dy.$$

Suppose the integral in this expression to be represented by N^1, then N^1 will become N (as it ought) in that particular case in which, the curve not sliding along the axis, A becomes $\frac{\pi}{2}$.

Now we may reason in respect to N^1 precisely as before in respect to N, and we shall obtain, by the same steps, the same expression for the surface in terms of N^1, as was then obtained in terms of N.

To find the Centre of Gravity of a Conchoidal Solid.

Suppose the solid included between P C and Q C (fig. 3.) to be divided into an infinite number of prismatic elements by planes perpendicular to P C, and perpendicular and parallel to A z; and let $m\,r$ (fig. 5.) represent one of these elements.

The VOLUME of this element is represented by

$$\overline{no} \times \overline{ns}$$

or by

$$\tfrac{1}{2} (mn + op) \overline{np} \,.\, \overline{ns}$$

or by

$$\tfrac{1}{2}(m\,n + o\,p)\,\overline{n\,r}$$

or by

$$\tfrac{1}{2}(u\,m + u\,o)\,.\,\overline{m\,q}\,.\sin\Delta\,\Theta\,.\cos\Delta\,\Theta\,. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

The MOMENTUM of the element about a plane passing through A x, and perpendicular to P C, (fig. 3.) is therefore represented by

$$\tfrac{1}{4}(u\,m + u\,o)^2\,\overline{m\,q}\,.\sin\Delta\,\Theta\,.\cos^2\Delta\,\Theta$$

or by

(the momentum of inertia of the plane $m\,q$) $\sin\Delta\,\Theta\,.\cos^2\Delta\,\Theta$.

Assuming then Θ to be measured from the plane $z\,y$, the momentum of the element $m\,r$ about the plane $z\,y$ is represented by

(momentum of inertia of elementary plane $m\,q$) $\sin\Theta\sin\Delta\,\Theta\cos^2\Delta\,\Theta$,

and the momentum of the same element about the plane $z\,x$ is represented by

(momentum of inertia of elementary plane $m\,q$) $\cos\Theta\sin\Delta\,\Theta\cos^2\Delta\,\Theta$.

Hence if we imagine two solids to be generated, one by the revolution of P C, without altering its dimensions, through the angle P C Q, and the other by the revolution of Q C through the same angle; and if we take I to represent the momentum of inertia of the plane P C; then will the momentum of the first solid about the plane $z\,y$, be represented by

$$\mathrm{I}\sin\Theta\sin\Delta\,\Theta\cos^2\Delta\,\Theta,$$

and that of the second by

$$\left(\mathrm{I} + \frac{d\,\mathrm{I}}{d\,\Theta}\Delta\,\Theta + \ldots.\right)\sin(\Theta + \Delta\,\Theta)\sin\Delta\,\Theta\cos^2\Delta\,\Theta.$$

Now the momentum of the elementary solid P C Q evidently lies between those of these elementary solids. Calling then the momentum of the whole solid, of which P C Q is an element, M_1, when estimated in respect to the plane $z\,y$, we have

$$\mathrm{I}\sin\Theta\cos^2\Delta\,\Theta\sin\Delta\,\Theta < \frac{d\,\mathrm{M}_1}{d\,\Theta}\Delta\,\Theta + \&c. \ldots. < \mathrm{I}\sin\Theta\cos^2\Delta\,\Theta\sin\Delta\,\Theta + \&c.$$

$$\therefore \mathrm{I}\sin\Theta\cos^2\Delta\,\Theta < \frac{d\,\mathrm{M}_1}{d\,\Theta}\cdot\frac{\Delta\,\Theta}{\sin\Delta\,\Theta} + \&c. \ldots. < \mathrm{I}\sin\Theta\cos^2\Delta\,\Theta + \&c.$$

And this is true for all values of $\Delta\,\Theta$.

$$\therefore \frac{d\,\mathrm{M}_1}{d\,\Theta} = \mathrm{I}\sin\Theta.$$

Similarly calling M_2 the moment of the whole solid about the plane $z\,x$

$$\frac{d\,\mathrm{M}_2}{d\,\Theta} = \mathrm{I}\cos\Theta$$

$$\therefore \mathrm{M}_1 = \int \mathrm{I}\sin\Theta\,d\,\Theta$$

$$M_2 = \int I \cos \Theta \, d\Theta$$

$$\therefore \text{ distance of centre of gravity from plane } z\,y = \frac{\int I \sin \Theta \, d\Theta}{\int M \, d\Theta}, \quad . \quad . \quad (4.)$$

$$\therefore \text{ distance of centre of gravity from plane } z\,x = \frac{\int I \cos \Theta \, d\Theta}{\int M \, d\Theta}. \quad . \quad . \quad (5.)$$

The generating curve has here been supposed to revolve about the axis A z, otherwise retaining its position upon it.

If we suppose P C to slide along the axis as it revolves (fig. 4.), the moment of the elementary solid P C Q about A z, and therefore the moments M_1 and M_2 of the whole solid about the planes $z\,x$ and $z\,y$ will remain unaltered.

Another dimension will however now have become necessary to determine the position of the centre of gravity; viz. its distance from a given point in the axis A z, measured along that axis.

Let V (fig. 4.) be the point where the generating curve intersects the axis A z; $\therefore$ by equation (3.) the momentum of the element $m\,r$ (fig. 5.) about a plane passing through V perpendicular to the axis A z is represented by

$$\tfrac{1}{2} V u \, (u\,m + u\,o) \, \overline{m\,q} \, . \sin \Delta \Theta \, . \cos \Delta \Theta ;$$

and assuming $V\,u = x$, and $u\,m$ (figs. 4 and 5.) $= y$, the momentum of the whole elementary solid generated by the revolution of P C through the angle $\Delta \Theta$ is represented by

$$\int\int x\,y \, dx \, dy \, . \sin \Delta \Theta \, . \cos \Delta \Theta .$$

And representing $\int\int x\,y \, dx \, dy$ by L, and reasoning as before, the moment of the whole solid about a plane perpendicular to A z passing through V is represented by

$$\int L \, d\Theta .$$

And if $A\,V = z$, the distance of the centre of gravity from A measured along the axis is represented by

$$z + \frac{\int L \, d\Theta}{\int M \, d\Theta} . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

To find the Centre of Gravity of a Conchoidal Surface.

Imagine the generating curve to describe, without altering its dimensions, an angle about the axis A z (fig. 3.), such that the circular arc described on this supposition by the point P may equal the element P Q of the length of the curve or ΔS; this angle will be represented by $\frac{\Delta S}{R}$. Moreover, the moment of the elementary surface thus generated about the plane $z\,y$ will be represented by

$$\int y^2 \sin \Theta \frac{\Delta S}{R} \, ds ,$$

where y is any ordinate of the generating curve at right angles to the axis A z, and s is taken to represent the length of the generating curve. Assuming then $\int y^2 \, ds$ or the moment of inertia of the perimeter of the curve to be represented by K, the moment of this imaginary surface about the plane $z\,y$ is represented by

$$\frac{K}{R} \sin \Theta \, \Delta S,$$

and, similarly, that about the plane $z\,x$ is represented by

$$\frac{K}{R} \cos \Theta \, \Delta S.$$

Conceiving now a similar elementary surface to be generated by the curve Q C without changing its dimensions, the two moments of that surface will be represented by

$$\frac{K + \Delta K}{R + \Delta R} \cos (\Theta + \Delta \Theta) \, \Delta S$$

and

$$\frac{K + \Delta K}{R + \Delta R} \sin (\Theta + \Delta \Theta) \, \Delta S.$$

Moreover, the moment of the actual element of the conchoidal surface evidently lies between the moments of these imaginary elements; as before, therefore, the whole moments of the conchoidal surface about the planes $z\,x$ and $z\,y$, being represented by N_1 and N_2,

$$\frac{d N_1}{d \Theta} = \frac{K}{R} \sin \Theta \frac{d S}{d \Theta}$$

$$\frac{d N_2}{d \Theta} = \frac{K}{R} \cos \Theta \frac{d S}{d \Theta}.$$

Similarly, if N_3 represent the moment of the surface about a vertical plane perpendicular to the axis A z, and passing through the point V; and if x be an abscissa to any point of the generating curve measured along the axis from that point; and if H represent the integral $\int x\,y\,d\,s$, taken in respect to the whole perimeter of the generating curve; then

$$\frac{d N_3}{d \Theta} = \frac{H}{R} \cdot \frac{d S}{d \Theta}.$$

The distances of the centre of gravity from the planes $z\,y$, $z\,x$, and $x\,y$, are then respectively

$$\frac{\int \frac{K}{R} \sin \Theta \, d S}{\int \frac{N}{R} d S} \quad \ldots \ldots \ldots \ldots \ldots \ldots \quad (7.)$$

$$\frac{\int \frac{K}{R} \cos \Theta \, d S}{\int \frac{N}{R} d S} \quad \ldots \ldots \ldots \ldots \ldots \ldots \quad (8.)$$

and

$$z + \frac{\int \frac{H}{R} d S}{\int \frac{N}{R} d S} \quad \ldots \ldots \ldots \ldots \ldots \ldots \quad (9.)$$

To determine the Area of the Surface and the Centre of Gravity of a Turbinated Shell, and the Volume and Centre of Gravity of its Contained Solid.

The generating curve of a turbinated shell remains similar to itself as it revolves; the statical moment of its perimeter varies therefore as the square, and the moment of inertia of its perimeter as the cube, of any of its linear dimensions. In like manner the statical moment of the area of its generating curve varies as the cube, and the moment of inertia as the fourth power, of any of its linear dimensions.

If therefore $C_1\ C_2\ C_3\ C_4\ C_5\ C_6$ represent certain constants determined by the geometrical conditions of the generating curve,

$$N = C_1 R^2 \quad K = C_2 R^3 \quad H = C_3 R^3$$
$$M = C_4 R^3 \quad I = C_5 R^4 \quad L = C_6 R^4.$$

Therefore the surface of the shell is represented by the integral

$$C_1 \int R\, dS. \qquad (10.)$$

The co-ordinates of the centre of gravity of the surface are represented by

$$\left.\begin{aligned} &\frac{C_2 \int R^2 \sin\Theta\, dS}{C_1 \int R\, dS}, \quad \frac{C_2 \int R^2 \cos\Theta\, dS}{C_1 \int R\, dS} \\ \text{and} \quad &z + \frac{C_3 \int R^3\, dS}{C_1 \int R\, dS}. \end{aligned}\right\} \qquad (11.)$$

And the volume of the contained solid is represented by

$$C_4 \int R^3\, d\Theta. \qquad (12.)$$

The co-ordinates of the centre of gravity of the contained solid are represented by

$$\left.\begin{aligned} &\frac{C_5 \int R^4 \sin\Theta\, d\Theta}{C_4 \int R^3\, d\Theta}, \quad \frac{C_5 \int R^4 \cos\Theta\, d\Theta}{C_4 \int R^3\, d\Theta} \\ &z + \frac{C_6 \int R^4\, d\Theta}{C_4 \int R^3\, d\Theta}. \end{aligned}\right\} \qquad (13.)$$

Now it has been shown that in shells R varies according to the law of the logarithmic spiral; so that

$$R = R_0\, \epsilon^{\Theta \cot A},$$

where R_0 is the value of R when $\Theta = 0$, and A is the constant angle which the radius vector of the spiral makes with its tangent, whether it be a plane curve or a curve of double curvature; whence it may readily be proved that

$$\frac{dS}{d\Theta} = R \operatorname{cosec} A. \qquad (14.)$$

Hence, substituting in the preceding formula, and integrating by the known rules, we obtain, for the surface of the shell, the expression

$$\tfrac{1}{2} C_1 R_0^2 \sec A\, (\epsilon^{2\Theta \cot A} - 1); \qquad (15.)$$

for the co-ordinates of the centre of gravity,

$$\frac{2\,C_2\,R_0}{C_1\,(\tan A + 9\cot A)} \cdot \frac{(3\cot A \sin\Theta - \cos\Theta)\,\epsilon^{3\,\Theta\cot A} + 1}{\epsilon^{2\,\Theta\cot A} - 1} \quad . \quad . \quad . \quad . \quad (16.)$$

$$\frac{2\,C_2\,R_0}{C_1\,(\tan A + 9\cot A)} \cdot \frac{(3\cot A\cos\Theta + \sin\Theta)\,\epsilon^{3\,\Theta\cot A} - 3\cot A}{\epsilon^{2\,\Theta\cot A} - 1} \quad . \quad . \quad . \quad (17.)$$

$$r_0\,(\epsilon^{(\Theta - 2n\pi)\cot A} - 1) + r_0\,\epsilon^{(\Theta - 2\pi)\cot A} \cdot \left(\frac{\epsilon^{-2n\pi\cot A} - 1}{\epsilon^{-2\pi\cot A} - 1}\right) + \frac{2}{3}\left(\frac{C_2 R_0}{C_1}\right)\left(\frac{\epsilon^{3\,\Theta\cot A} - 1}{\epsilon^{2\,\Theta\cot A} - 1}\right). \quad (18.)$$

Observing that r_0 being taken to represent the initial length of the lesser diameter* V T, of the generating curve, $r_0\,\epsilon^{\Theta\cot A}$ will represent the length of that diameter after the generating curve has revolved through the angle Θ, and $r_0\,\epsilon^{(\Theta - 2\pi)\cot A}$ will represent the width of the next preceding whorl of the shell, measured in the direction of this diameter produced; and the sum of the widths of all the preceding whorls, supposed to be n in number, and measured in this direction, will be represented by

$$\Sigma^n{}_1\, r_0\,\epsilon^{(\Theta - 2n\pi)\cot A}.$$

Moreover, that the lesser diameter sliding along the axis, as the curve revolves through any angle, a distance precisely equal to that by which the diameter increases, it follows that the distance from the edge of the last or nth of the preceding whorls, measured in this direction, to the origin is represented by

$$r_0\,(\epsilon^{(\Theta - 2n\pi)\cot A} - 1).$$

So that z_1† is represented by the formula

$$r_0\,(\epsilon^{(\Theta - 2n\pi)\cot A} - 1) + \Sigma^n{}_1\, r_0\,\epsilon^{(\Theta - 2n\pi)\cot A}.$$

Integrating the formula (12.), having substituted for the value of R, we find for the VOLUME of the solid contained by the shell the expression

$$\frac{1}{3}\,C_4\,R_0{}^3\tan A\,(\epsilon^{3\,\Theta\cot A} - 1).‡ \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19.)$$

And integrating the formula (13.), the co-ordinates of the centre of gravity of the contained solid are found to be

$$\frac{3\,C_5\,R_0}{C_4\,(\tan A + 16\cot A)} \cdot \frac{(4\cot A\sin\Theta - \cos\Theta)\,\epsilon^{4\,\Theta\cot A} + 1}{\epsilon^{3\,\Theta\cot A} - 1} \quad . \quad . \quad . \quad . \quad (20.)$$

$$\frac{3\,C_5\,R_0}{C_4\,(\tan A + 16\cot A)} \cdot \frac{(4\cot A\cos\Theta + \sin\Theta)\,\epsilon^{4\,\Theta\cot A} - 4\cot A}{\epsilon^{3\,\Theta\cot A} - 1} \quad . \quad . \quad . \quad (21.)$$

* When the whorls partially overlap one another, this diameter is to be understood to extend only across that portion of the generating curve which actually generates the chamber of the shell, and which is not interfered with by the preceding whorl. In these cases, then, it will only be a portion of what would be the shorter diameter of the generating curve, if that curve were completed.

† In the case in which the generating curve does not slide upon the axis as it revolves, $z_1 = 0$.

‡ In the case of turbinated shells R_0 may be considered extremely small with respect to any existing dimensions, and Θ exceedingly great, so that the formula 19. being taken to represent the whole capacity of the shell, becomes in this case $\frac{1}{3}\,C_4\,R_0{}^3\,\epsilon^{3\,\Theta\cot A}$, and varies as R^3.

$$r_0\left(\varepsilon^{(\Theta-2n\pi)\cot A}-1\right)+r_0\varepsilon^{(\Theta-2\pi)\cot A}\cdot\left(\frac{\varepsilon^{-2n\pi\cot A}-1}{\varepsilon^{-2\pi}-1}\right)+\frac{3}{4}\left(\frac{C_6R_0}{C_4}\right)\left(\varepsilon^{4\Theta\cot A}-1.\right)\quad(22.)$$

To determine the Polar Equation to the surface of a Turbinated Shell.

Let m (fig. 4.) be any point in the surface of the shell, and let the equation to the curve V Q T, imagined to be in the act of generating the point m, be

$$y_1 = f(B, x_1)$$

where B is an arbitrary constant representing a linear dimension of the curve, and therefore varying according to the law of the logarithmic spiral, so that it may be represented by the formula

$$B = B_0\,\varepsilon^{\Theta\cot A},$$

where B_0 is the initial value of B. Suppose the abscissæ of the curve to be measured along the axis V T from V, so that V u and $u\,m$ are co-ordinates of m. Let A $m = \varrho$, m A $z = \Phi$, A $u = \varrho\cos\Phi = z_1 + x_1$, $u\,m = \varrho\sin\Phi = y_1$

$$\therefore \varrho\sin\Phi = f(B_0\,\varepsilon^{\Theta\cot A}, \varrho\cos\Phi - z_1)\quad(23.)$$

or substituting for z_1 its value

$$\varrho\sin\Phi = f\left\{B_0\,\varepsilon^{\Theta\cot A}, \varrho\cos\Phi - r_0\left(\varepsilon^{(\Theta-2n\pi)\cot A}-1\right) - r_0\varepsilon^{(\Theta-2n\pi)\cot A}\left(\frac{\varepsilon^{-2n\pi\cot A}-1}{\varepsilon^{-2\pi\cot A}-1}\right)\right\}\quad(24.)$$

From the above may readily be determined the equation to the surface of a shell between the rectangular co-ordinates x, y, z. Observing that $\Theta - 2\,n\,\pi = \tan^{-1}\frac{x}{y}$, and substituting, we obtain

$$(x^2+y^2)^{\frac{1}{2}} = f\left\{B_0\,\varepsilon^{\left(2n\pi+\tan^{-1}\frac{x}{y}\right)\cot A}, z - r_0\left(\varepsilon^{\tan^{-1}\frac{x}{y}\cot A}-1\right) - r_0\varepsilon^{\tan^{-1}\frac{x}{y}\cot A}\cdot\left(\frac{\varepsilon^{-2n\pi\cot A}-1}{\varepsilon^{-2\pi\cot A}-1}\right)\right\}\quad(25.)$$

The values of the constants $C_1\,C_2\,C_3\,C_4\,C_5\,C_6$ are dependent upon the geometrical form of the generating curve in each particular shell; the constants R_0 r_0 and B_0 on its dimensions at the point where the generation of the shell is supposed to commence.

The constant A is independent of the form and dimensions of the generating curve. It depends simply upon the law of that particular logarithmic spiral which is affected by that species of shell.

To determine the Constant Angle of the Spiral affected by any given Shell.

The common ratio of the geometrical progression according to which the widths of successive whorls increase being determined by actual admeasurement and represented by λ, we have the equation

$$\varepsilon^{2\pi\cot A} = \lambda$$

$$\therefore A = \tan^{-1}\left(\frac{2\pi}{\log_\varepsilon\lambda}\right)\quad(26.)$$

XVIII. *Contributions to the Physiology of Vision.—Part the First. On some remarkable, and hitherto unobserved, Phenomena of Binocular Vision.* By CHARLES WHEATSTONE, *F.R.S., Professor of Experimental Philosophy in King's College, London.*

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§ 1.

WHEN an object is viewed at so great a distance that the optic axes of both eyes are sensibly parallel when directed towards it, the perspective projections of it, seen by each eye separately, are similar, and the appearance to the two eyes is precisely the same as when the object is seen by one eye only. There is, in such case, no difference between the visual appearance of an object in relief and its perspective projection on a plane surface; and hence pictorial representations of distant objects, when those circumstances which would prevent or disturb the illusion are carefully excluded, may be rendered such perfect resemblances of the objects they are intended to represent as to be mistaken for them; the Diorama is an instance of this. But this similarity no longer exists when the object is placed so near the eyes that to view it the optic axes must converge; under these conditions a different perspective projection of it is seen by each eye, and these perspectives are more dissimilar as the convergence of the optic axes becomes greater. This fact may be easily verified by placing any figure of three dimensions, an outline cube for instance, at a moderate distance before the eyes, and while the head is kept perfectly steady, viewing it with each eye successively while the other is closed. Plate XI. fig. 13. represents the two perspective projections of a cube; *b* is that seen by the right eye, and *a* that presented to the left eye; the figure being supposed to be placed about seven inches immediately before the spectator.

The appearances, which are by this simple experiment rendered so obvious, may be easily inferred from the established laws of perspective; for the same object in relief is, when viewed by a different eye, seen from two points of sight at a distance from each other equal to the line joining the two eyes. Yet they seem to have escaped the attention of every philosopher and artist who has treated of the subjects of vision and perspective. I can ascribe this inattention to a phenomenon leading to the important and curious consequences, which will form the subject of the present communication, only to this circumstance; that the results being contrary to a principle which was very generally maintained by optical writers, viz. that objects can

be seen single only when their images fall on corresponding points of the two retinæ, an hypothesis which will be hereafter discussed, if the consideration ever arose in their minds, it was hastily discarded under the conviction, that if the pictures presented to the two eyes are under certain circumstances dissimilar, their differences must be so small that they need not be taken into account.

It will now be obvious why it is impossible for the artist to give a faithful representation of any near solid object, that is, to produce a painting which shall not be distinguished in the mind from the object itself. When the painting and the object are seen with both eyes, in the case of the painting two *similar* pictures are projected on the retinæ, in the case of the solid object the pictures are *dissimilar*; there is therefore an essential difference between the impressions on the organs of sensation in the two cases, and consequently between the perceptions formed in the mind; the painting therefore cannot be confounded with the solid object.

After looking over the works of many authors who might be expected to have made some remarks relating to this subject, I have been able to find but one, which is in the Trattato della Pittura of Leonardo da Vinci*. This great artist and ingenious philosopher observes, "that a painting, though conducted with the greatest art and finished to the last perfection, both with regard to its contours, its lights, its shadows and its colours, can never show a relievo equal to that of the natural objects, unless these be viewed at a distance and with a single eye. For," says he, "if an object C (Plate X. fig. 1.) be viewed by a single eye at A, all objects in the space behind it, included as it were in a shadow E C F cast by a candle at A, are invisible to the eye at A; but when the other eye at B is opened, part of these objects become visible to it; those only being hid from both eyes that are included, as it were, in the double shadow C D, cast by two lights at A and B, and terminated in D, the angular space E D G beyond D being always visible to both eyes. And the hidden space C D is so much the shorter, as the object C is smaller and nearer to the eyes. Thus the object C seen with both eyes becomes, as it were, transparent, according to the usual definition of a transparent thing; namely, that which hides nothing beyond it. But this cannot happen when an object, whose breadth is bigger than that of the pupil, is viewed by a single eye. The truth of this observation is therefore evident, because a painted figure intercepts all the space behind its apparent place, so as to preclude the eyes from the sight of every part of the imaginary ground behind it."

Had Leonardo da Vinci taken, instead of a sphere, a less simple figure for the purpose of his illustration, a cube for instance, he would not only have observed that the object obscured from each eye a different part of the more distant field of view, but the fact would also perhaps have forced itself upon his attention, that the object itself presented a different appearance to each eye. He failed to do this, and no subsequent writer within my knowledge has supplied the omission; the projection of two

* See also a Treatise of Painting, p. 178. London, 1721; and Dr. Smith's Complete System of Optics, vol. ii. r. 244, where the passage is quoted.

obviously dissimilar pictures on the two retinæ when a single object is viewed, while the optic axes converge, must therefore be regarded as a new fact in the theory of vision.

§ 2.

It being thus established that the mind perceives an object of three dimensions by means of the two dissimilar pictures projected by it on the two retinæ, the following question occurs: What would be the visual effect of simultaneously presenting to each eye, instead of the object itself, its projection on a plane surface as it appears to that eye? To pursue this inquiry it is necessary that means should be contrived to make the two pictures, which must necessarily occupy different places, fall on similar parts of both retinæ. Under the ordinary circumstances of vision the object is seen at the concourse of the optic axes, and its images consequently are projected on similar parts of the two retinæ; but it is also evident that two exactly similar objects may be made to fall on similar parts of the two retinæ, if they are placed one in the direction of each optic axis, at equal distances before or beyond their intersection.

Fig. 2. represents the usual situation of an object at the intersection of the optic axes. In fig. 3. the similar objects are placed in the direction of the optic axes before their intersection, and in fig. 4. beyond it. In all these three cases the mind perceives but a single object, and refers it to the place where the optic axes meet. It will be observed, that when the eyes converge beyond the objects, as in fig. 3., the right hand object is seen by the right eye, and the left hand object by the left eye; while when the axes converge nearer than the objects, the right hand object is seen by the left eye, and conversely. As both of these modes of vision are forced and unnatural, eyes unaccustomed to such experiments require some artificial assistance. If the eyes are to converge beyond the objects, this may be afforded by a pair of tubes (fig. 5.) capable of being inclined towards each other at various angles, so as to correspond with the different convergences of the optic axes. If the eyes are to converge at a nearer distance than that at which the objects are placed, a box (fig. 6.) may be conveniently employed; the objects a a' are placed distant from each other, on a stand capable of being moved nearer the eyes if required, and the optic axes being directed towards them will cross at c, the aperture b b' allowing the visual rays from the right hand object to reach the left eye, and those from the left hand object to fall on the right eye; the coincidence of the images may be facilitated by placing the point of a needle at the point of intersection of the optic axes c, and fixing the eyes upon it. In both these instruments (figs. 5. and 6.) the lateral images are hidden from view, and much less difficulty occurs in making the images unite than when the naked eyes are employed.

Now if, instead of placing two exactly similar objects to be viewed by the eyes in either of the modes above described, the two perspective projections of the same solid object be so disposed, the mind will still perceive the object to be single, but instead of a representation on a plane surface, as each drawing appears to be when separately

viewed by that eye which is directed towards it, the observer will perceive a figure of three dimensions, the exact counterpart of the object from which the drawings were made. To make this matter clear I will mention one or two of the most simple cases.

If two vertical lines near each other, but at different distances from the spectator, be regarded first with one eye and then with the other, the distance between them when referred to the same plane will appear different; if the left hand line be nearer to the eyes, the distance as seen by the left eye will be less than the distance as seen by the right eye; fig. 7. will render this evident; $a\,a'$ are vertical sections of the two original lines, and $b\,b'$ the plane to which their projections are referred. Now if the two lines be drawn on two pieces of card, at the respective distances at which they appear to each eye, and these cards be afterwards viewed by either of the means above directed, the observer will no longer see two lines on a plane surface, as each card separately shows; but two lines will appear, one nearer to him than the other, precisely as the original vertical lines themselves. Again, if a straight wire be held before the eyes in such a position that one of its ends shall be nearer to the observer than the other is, each eye separately referring it to a plane perpendicular to the common axis, will see a line differently inclined; and then if lines having the same apparent inclinations be drawn on two pieces of card, and be presented to the eyes as before directed, the real position of the original line will be correctly perceived by the mind.

In the same manner the most complex figures of three dimensions may be accurately represented to the mind, by presenting their two perspective projections to the two retinæ. But I shall defer these more perfect experiments until I describe an instrument which will enable any person to observe all the phenomena in question with the greatest ease and certainty.

In the instruments above described the optic axes converge to some point in a plane before or beyond that in which the objects to be seen are situated. The adaptation of the eye, which enables us to see distinctly at different distances, and which habitually accompanies every different degree of convergence of the optic axes, does not immediately adjust itself to the new and unusual condition; and to persons not accustomed to experiments of this kind, the pictures will either not readily unite, or will appear dim and confused. Besides this, no object can be viewed according to either mode when the drawings exceed in breadth the distance of the two points of the optic axes in which their centres are placed.

These inconveniences are removed by the instrument I am about to describe; the two pictures (or rather their reflected images) are placed in it at the true concourse of the optic axes, the focal adaptation of the eye preserves its usual adjustment, the appearance of lateral images is entirely avoided, and a large field of view for each eye is obtained. The frequent reference I shall have occasion to make to this instrument, will render it convenient to give it a specific name, I therefore propose that it be called a Stereoscope, to indicate its property of representing solid figures.

§ 3.

The stereoscope is represented by figs. 8. and 9; the former being a front view, and the latter a plan of the instrument. A A′ are two plane mirrors, about four inches square, inserted in frames, and so adjusted that their backs form an angle of 90° with each other; these mirrors are fixed by their common edge against an upright B, or which was less easy to represent in the drawing, against the middle line of a vertical board, cut away in such manner as to allow the eyes to be placed before the two mirrors. C C′ are two sliding boards, to which are attached the upright boards D D′, which may thus be removed to different distances from the mirrors. In most of the experiments hereafter to be detailed, it is necessary that each upright board shall be at the same distance from the mirror which is opposite to it. To facilitate this double adjustment, I employ a right and a left-handed wooden screw, *r l*; the two ends of this compound screw pass through the nuts *e e′*, which are fixed to the lower parts of the upright boards D D′, so that by turning the screw pin *p* one way the two boards will approach, and by turning it the other they will recede from each other, one always preserving the same distance as the other from the middle line *f*. E E′ are pannels, to which the pictures are fixed in such manner that their corresponding horizontal lines shall be on the same level: these pannels are capable of sliding backwards and forwards in grooves on the upright boards D D′. The apparatus having been described, it now remains to explain the manner of using it. The observer must place his eyes as near as possible to the mirrors, the right eye before the right hand mirror, and the left eye before the left hand mirror, and he must move the sliding pannels E E′ to or from him until the two reflected images coincide at the intersection of the optic axes, and form an image of the same apparent magnitude as each of the component pictures. The pictures will indeed coincide when the sliding pannels are in a variety of different positions, and consequently when viewed under different inclinations of the optic axes; but there is only one position in which the binocular image will be immediately seen single, of its proper magnitude, and without fatigue to the eyes, because in this position only the ordinary relations between the magnitude of the pictures on the retina, the inclination of the optic axes, and the adaptation of the eye to distinct vision at different distances are preserved. The alteration in the apparent magnitude of the binocular images, when these usual relations are disturbed, will be discussed in another paper of this series, with a variety of remarkable phenomena depending thereon. In all the experiments detailed in the present memoir I shall suppose these relations to remain undisturbed, and the optic axes to converge about six or eight inches before the eyes.

If the pictures are all drawn to be seen with the same inclination of the optic axes, the apparatus may be simplified by omitting the screw *r l* and fixing the upright boards D D′ at the proper distances. The sliding pannels may also be dispensed with, and the drawings themselves be made to slide in the grooves.

§ 4.

A few pairs of outline figures, calculated to give rise to the perception of objects of three dimensions when placed in the stereoscope in the manner described, are represented from figs. 10. to 20. They are one half the linear size of the figures actually employed. As the drawings are reversed by reflection in the mirrors, I will suppose these figures to be the reflected images to which the eyes are directed in the apparatus; those marked *b* being seen by the right eye, and those marked *a* by the left eye. The drawings, it has been already explained, are two different projections of the same object seen from two points of sight, the distance between which is equal to the interval between the eyes of the observer; this interval is generally about $2\frac{1}{2}$ inches.

a and *b*, fig. 10. will, when viewed in the stereoscope, present to the mind a line in the vertical plane, with its lower end inclined towards the observer. If the two component lines be caused to turn round their centres equally in opposite directions, the resultant line will, while it appears to assume every degree of inclination to the referent plane, still seem to remain in the same vertical plane.

Fig. 11. A series of points all in the same horizontal plane, but each towards the right hand successively nearer the observer.

Fig. 12. A curved line intersecting the referent plane, and having its convexity towards the observer.

Fig. 13. A cube.

Fig. 14. A cone, having its axis perpendicular to the referent plane, and its vertex towards the observer.

Fig. 15. The frustum of a square pyramid; its axis perpendicular to the referent plane, and its base furthest from the eye.

Fig. 16. Two circles at different distances from the eyes, their centres in the same perpendicular, forming the outline of the frustum of a cone.

The other figures require no observation.

For the purposes of illustration I have employed only outline figures, for had either shading or colouring been introduced it might be supposed that the effect was wholly or in part due to these circumstances, whereas by leaving them out of consideration no room is left to doubt that the entire effect of relief is owing to the simultaneous perception of the two monocular projections, one on each retina. But if it be required to obtain the most faithful resemblances of real objects, shadowing and colouring may properly be employed to heighten the effects. Careful attention would enable an artist to draw and paint the two component pictures, so as to present to the mind of the observer, in the resultant perception, perfect identity with the object represented. Flowers, crystals, busts, vases, instruments of various kinds, &c., might thus be represented so as not to be distinguished by sight from the real objects themselves.

It is worthy of remark, that the process by which we thus become acquainted with the real forms of solid objects, is precisely that which is employed in descriptive geometry, an important science we owe to the genius of Monge, but which is little studied or known in this country. In this science, the position of a point, a right line or a curve, and consequently of any figure whatever, is completely determined by assigning its projections on two fixed planes, the situations of which are known, and which are not parallel to each other. In the problems of descriptive geometry the two referent planes are generally assumed to be at right angles to each other, but in binocular vision the inclination of these planes is less according as the angle made at the concourse of the optic axes is less; thus the same solid object is represented to the mind by different pairs of monocular pictures, according as they are placed at a different distance before the eyes, and the perception of these differences (though we seem to be unconscious of them) may assist in suggesting to the mind the distance of the object. The more inclined to each other the referent planes are, with the greater accuracy are the various points of the projections referred to their proper places; and it appears to be a useful provision that the real forms of those objects which are nearest to us are thus more determinately apprehended than those which are more distant.

§ 5.

A very singular effect is produced when the drawing originally intended to be seen by the right eye is placed at the left hand side of the stereoscope, and that designed to be seen by the left eye is placed on its right hand side. A figure of three dimensions, as bold in relief as before, is perceived, but it has a different form from that which is seen when the drawings are in their proper places. There is a certain relation between the proper figure and this, which I shall call its *converse* figure. Those points which are nearest the observer in the proper figure are the most remote from him in the converse figure, and *vice versâ*, so that the figure is, as it were, inverted; but it is not an exact inversion, for the near parts of the converse figure appear smaller, and the remote parts larger than the same parts before the inversion. Hence the drawings which, properly placed, occasion a cube to be perceived, when changed in the manner described, represent the frustum of a square pyramid with its base remote from the eye: the cause of this is easy to understand.

This conversion of relief may be shown by all the pairs of drawings from fig. 10. to 19. In the case of simple figures like these the converse figure is as readily apprehended as the original one, because it is generally a figure of as frequent occurrence; but in the case of a more complicated figure, an architectural design, for instance, the mind, unaccustomed to perceive its converse, because it never occurs in nature, can find no meaning in it.

§ 6.

The same image is depicted on the retina by an object of three dimensions as by its projection on a plane surface, provided the point of sight remain in both cases the same. There should be, therefore, no difference in the binocular appearance of two drawings, one presented to each eye, and of two real objects so presented to the two eyes that their projections on the retina shall be the same as those arising from the drawings. The following experiments will prove the justness of this inference.

I procured several pairs of skeleton figures, i. e. outline figures of three dimensions, formed either of iron wire or of ebony beading about one tenth of an inch in thickness. The pair I most frequently employed consisted of two cubes, whose sides were three inches in length. When I placed these skeleton figures on stands before the two mirrors of the stereoscope, the following effects were produced, according as their relative positions were changed. 1st. When they were so placed that the pictures which their reflected images projected on the two retinæ were precisely the same as those which would have been projected by a cube placed at the concourse of the optic axes, a cube in relief appeared before the eyes. 2ndly. When they were so placed that their reflected images projected exactly similar pictures on the two retinæ, all effect of relief was destroyed, and the compound appearance was that of an outline representation on a plane surface. 3rdly. When the cubes were so placed that the reflected image of one projected on the left retina the same picture as in the first case was projected on the right retina, and conversely, the converse figure in relief appeared.

§ 7.

If a symmetrical object, that is one whose right and left sides are exactly similar to each other but inverted, be placed so that any point in the plane which divides it into these two halves is equally distant from the two eyes, its two monocular projections are, it is easy to see, inverted fac-similes of each other. Thus fig. 15, *a* and *b* are symmetrical monocular projections of the frustum of a four-sided pyramid, and figs. 13. 14. 16. are corresponding projections of other symmetrical objects. This being kept in view, I will describe an experiment which, had it been casually observed previous to the knowledge of the principles developed in this paper, would have appeared an inexplicable optical illusion.

M and M′ (fig. 21.) are two mirrors, inclined so that their *faces* form an angle of 90° with each other. Between them in the bisecting plane is placed a plane outline figure, such as fig. 15 *a*, made of card all parts but the lines being cut away, or of wire. A reflected image of this outline, placed at A, will appear behind each mirror at B and B′, and one of these images will be the inversion of the other. If the eyes be made to converge at C, it is obvious that these two reflected images will fall on corresponding parts of the two retinæ, and a figure of three dimensions will be perceived; if the outline placed in the bisecting plane be reversed, the converse skeleton

form will appear; in both these experiments we have the singular phenomenon of the conversion of a single plane outline into a figure of three dimensions. To render the binocular object more distinct, concave lenses may be applied to the eyes; and to prevent the two lateral images from being seen, screens may be placed at D and D'.

§ 8.

An effect of binocular perspective may be remarked in a plate of metal, the surface of which has been made smooth by turning it in a lathe. When a single candle is brought near such a plate, a line of light appears standing out from it, one half being above, and the other half below the surface; the position and inclination of this line changes with the situation of the light and of the observer, but it always passes through the centre of the plate. On closing the left eye the relief disappears, and the luminous line coincides with one of the diameters of the plate; on closing the right eye the line appears equally in the plane of the surface, but coincides with another diameter; on opening both eyes it instantly starts into relief*. The case here is exactly analogous to the vision of two inclined lines (fig. 10.) when each is presented to a different eye in the stereoscope. It is curious, that an effect like this, which must have been seen thousands of times, should never have attracted sufficient attention to have been made the subject of philosophic observation. It was one of the earliest facts which drew my attention to the subject I am now treating.

Dr. Smith† was very much puzzled by an effect of binocular perspective which he observed, but was unable to explain. He opened a pair of compasses, and while he held the joint in his hand, and the points outwards and equidistant from his eyes, and somewhat higher than the joint, he looked at a more distant point; the compasses appeared double. He then compressed the legs until the two inner points coincided; having done this the two inner legs also entirely coincided, and bisected the angle formed by the outward ones, appearing longer and thicker than they did, and reaching from the hand to the remotest object in view. The explanation offered by Dr. Smith accounts only for the coincidence of the points of the compasses, not for that of the entire leg. The effect in question is best seen by employing a pair of straight wires, about a foot in length. A similar observation, made with two flat rulers, and afterwards with silk threads, induced Dr. Wells to propose a new theory of visible direction in order to explain it, so inexplicable did it seem to him by any of the received theories.

* The luminous line seen by a single eye arises from the reflection of the light from each of the concentric circles produced in the operation of turning; when the plate is not large the arrangement of these successive reflections does not differ from a straight line.

† System of Optics, vol. ii. p. 388. and r. 526.

§ 9.

The preceding experiments render it evident that there is an essential difference in the appearance of objects when seen with two eyes, and when only one eye is employed, and that the most vivid belief of the solidity of an object of three dimensions arises from two different perspective projections of it being simultaneously presented to the mind. How happens it then, it may be asked, that persons who see with only one eye form correct notions of solid objects, and never mistake them for pictures? and how happens it also, that a person having the perfect use of both eyes, perceives no difference in objects around him when he shuts one of them? To explain these apparent difficulties, it must be kept in mind, that although the simultaneous vision of two dissimilar pictures suggests the relief of objects in the most vivid manner, yet there are other signs which suggest the same ideas to the mind, which, though more ambiguous than the former, become less liable to lead the judgment astray in proportion to the extent of our previous experience. The vividness of relief arising from the projection of two dissimilar pictures, one on each retina, becomes less and less as the object is seen at a greater distance before the eyes, and entirely ceases when it is so distant that the optic axes are parallel while regarding it. We see with both eyes all objects beyond this distance precisely as we see near objects with a single eye; for the pictures on the two retinæ are then exactly similar, and the mind appreciates no difference whether two identical pictures fall on corresponding parts of the two retinæ, or whether one eye is impressed with only one of these pictures. A person deprived of the sight of one eye sees therefore all external objects, near and remote, as a person with both eyes sees remote objects only, but that vivid effect arising from the binocular vision of near objects is not perceived by the former; to supply this deficiency he has recourse unconsciously to other means of acquiring more accurate information. The motion of the head is the principal means he employs. That the required knowledge may be thus obtained will be evident from the following considerations. The mind associates with the idea of a solid object every different projection of it which experience has hitherto afforded; a single projection may be ambiguous, from its being also one of the projections of a picture, or of a different solid object; but when different projections of the same object are successively presented, they cannot all belong to another object, and the form to which they belong is completely characterized. While the object remains fixed, at every movement of the head it is viewed from a different point of sight, and the picture on the retina consequently continually changes.

Every one must be aware how greatly the perspective effect of a picture is enhanced by looking at it with only one eye, especially when a tube is employed to exclude the vision of adjacent objects, whose presence might disturb the illusion. Seen under such circumstances from the proper point of sight, the picture projects the same lines, shades and colours on the retina, as the more distant scene which it represents

would do were it substituted for it. The appearance which would make us certain that it is a picture is excluded from the sight, and the imagination has room to be active. Several of the older writers erroneously attributed this apparent superiority of monocular vision to the concentration of the visual power in a single eye *.

There is a well-known and very striking illusion of perspective which deserves a passing remark, because the reason of the effect does not appear to be generally understood. When a perspective of a building is projected on a horizontal plane, so that the point of sight is in a line greatly inclined towards the plane, the building appears to a single eye placed at the point of sight to be in bold relief, and the illusion is almost as perfect as in the binocular experiments described in §§ 2, 3, 4. This effect wholly arises from the unusual projection, which suggests to the mind more readily the object itself than the drawing of it; for we are accustomed to see real objects in almost every point of view, but perspective representations being generally made in a vertical plane with the point of sight in a line perpendicular to the plane of projection, we are less familiar with the appearance of other projections. Any other unusual projection will produce the same effect.

§ 10.

If we look with a single eye at the drawing of a solid geometrical figure, it may be imagined to be the representation of either of two dissimilar solid figures, the figure intended to be represented, or its converse figure (§ 5.). If the former is a very usual, and the latter a very unusual figure, the imagination will fix itself on the original without wandering to the converse figure; but if both are of ordinary occurrence, which is generally the case with regard to simple forms, a singular phenomenon takes place; it is perceived at one time distinctly as one of these figures, at another time as the other, and while one figure continues it is not in the power of the will to change it immediately.

The same phenomenon takes place, though less decidedly, when the drawing is seen with both eyes. Many of my readers will call to mind the puzzling effect of some of the diagrams annexed to the problems of the eleventh book of Euclid; which, when they were attentively looked at, changed in an arbitrary manner from one solid figure to another, and would obstinately continue to present the converse figures when the real figures alone were wanted. This perplexing illusion must be of common occurrence, but I have only found one recorded observation relating to the subject. It is by Professor Necker of Geneva, and I shall quote it in his own words from the Philosophical Magazine, Third Series, vol. i. p. 337.

"The object I have now to call your attention to is an observation which has often

* "We see more exquisitely with one eye shut than with both, because the vital spirits thus unite themselves the more, and become the stronger: for we may find by looking in a glass whilst we shut one eye, that the pupil of the other dilates."—Lord Bacon's Works, Sylva Sylvarum, art. Vision.

occurred to me while examining figures and engraved plates of crystalline forms; I mean a sudden and involuntary change in the apparent position of a crystal or solid represented in an engraved figure. What I mean will be more easily understood from the figure annexed (fig. 22.). The rhomboid A X is drawn so that the solid angle A should be seen the nearest to the spectator, and the solid angle X the farthest from him, and that the face A C D B should be the foremost, while the face X D C is behind. But in looking repeatedly at the same figure, you will perceive that at times the apparent position of the rhomboid is so changed that the solid angle X will appear the nearest, and the solid angle A the farthest; and that the face A C D B will recede behind the face X D C, which will come forward, which effect gives to the whole solid a quite contrary apparent inclination."

Professor Necker attributes this alteration of appearance, not to a mental operation, but to an involuntary change in the adjustment of the eye for obtaining distinct vision. He supposed that whenever the point of distinct vision on the retina is directed on the angle A, for instance, this angle seen more distinctly than the others is naturally supposed to be nearer and foremost, while the other angles seen indistinctly are supposed to be farther and behind, and that the reverse takes place when the point of distinct vision is brought to bear on the angle X.

That this is not the true explanation, is evident from three circumstances: in the first place, the two points A and X being both at the same distance from the eyes, the same alteration of adjustment which would make one of them indistinct would make the other so; secondly, the figure will undergo the same changes whether the focal distance of the eye be adjusted to a point before or beyond the plane in which the figure is drawn; and thirdly, the change of figure frequently occurs while the eye continues to look at the same angle. The effect seems entirely to depend on our mental contemplation of the figure intended to be represented, or of its converse. By following the lines with the eye with a clear idea of the solid figure we are describing, it may be fixed for any length of time; but it requires practice to do this or to change the figure at will. As I have before observed, these effects are far more obvious when the figures are regarded with one eye only.

No illusion of this kind can take place when an object of three dimensions is seen with both eyes while the optic axes make a sensible angle with each other, because the appearance of the two dissimilar images, one to each eye, prevents the possibility of mistake. But if we regard an object at such a distance that its two projections are sensibly identical, and if this projection be capable of a double interpretation, the illusion may occur. Thus a placard on a pole carried in the streets, with one of its sides inclined towards the observer, will, when he is distant from it, frequently appear inclined in a contrary direction. Many analogous instances might be adduced, but this will suffice to call others to mind; it must however be observed, that when shadows, or other means capable of determining the judgement are present, these fallacies do not arise.

§ 11.

The same indetermination of judgement which causes a drawing to be perceived by the mind at different times as two different figures, frequently gives rise to a false perception when objects in relief are regarded with a single eye. The apparent conversion of a cameo into an intaglio, and of an intaglio into a cameo, is a well-known instance of this fallacy in vision; but the fact does not appear to me to have been correctly explained, nor the conditions under which it occurs to have been properly stated.

This curious illusion, which has been the subject of much attention, was first observed at one of the early meetings of the Royal Society*. Several of the members looking through a compound microscope of a new construction at a guinea, some of them imagined the image to be depressed, while others thought it to be embossed, as it really was. Professor Gmelin, of Wurtemburg, published a paper on the same subject in the Philosophical Transactions for 1745; his experiments were made with telescopes and compound microscopes which inverted the images; and he observed that the conversion of relief appeared in some cases and not in others, at some times and not at others, and to some eyes also and not to others. He endeavoured to ascertain some of the conditions of the two appearances; "but why these things should so happen," says he, "I do not pretend to determine."

Sir David Brewster accounts for the fallacy in the following manner†:—"A hollow seal being illuminated by a window or a candle, its shaded side is of course on the same side with the light. If we now invert the seal with one or more lenses, so that it may look in the opposite direction, it will appear to the eye with the shaded side furthest from the window. But as we know that the window is still on our left hand, and as every body with its shaded side furthest from the light must necessarily be convex or protuberant, we immediately believe that the hollow seal is now a cameo or bas-relief. The proof which the eye thus receives of the seal being raised, overcomes the evidence of its being hollow, derived from our actual knowledge and from the sense of touch. In this experiment the deception takes place from our knowing the real direction of the light which falls on the seal; for if the place of the window, with respect to the seal, had been inverted as well as the seal itself, the illusion could not have taken place. The illusion, therefore, under our consideration is the result of an operation of our own minds, whereby we judge of the forms of bodies by the knowledge we have acquired of light and shadow. Hence the illusion depends on the accuracy and extent of our knowledge on this subject; and while some persons are under its influence, others are entirely insensible to it."

These considerations do not fully explain the phenomenon, for they suppose that the image must be inverted, and that the light must fall in a particular direction; but the conversion of relief will still take place when the object is viewed through an

* Birch's History, vol. ii. p. 348.

† Natural Magic, p. 100.

open tube without any lenses to invert it, and also when it is equally illuminated in all parts. The true explanation I believe to be the following. If we suppose a cameo and an intaglio of the same object, the elevations of the one corresponding exactly to the depressions of the other, it is easy to show that the projection of either on the retina is sensibly the same. When the cameo or the intaglio is seen with both eyes, it is impossible to mistake an elevation for a depression, for reasons which have been already amply explained; but when either is seen with one eye only, the most certain guide of our judgement, viz. the presentation of a different picture to each eye, is wanting; the imagination therefore supplies the deficiency, and we conceive the object to be raised or depressed according to the dictates of this faculty. No doubt in such cases our judgement is in a great degree influenced by accessory circumstances, and the intaglio or the relief may sometimes present itself according to our previous knowledge of the direction in which the shadows ought to appear; but the real cause of the phenomenon is to be found in the indetermination of the judgement arising from our more perfect means of judging being absent.

Observers with the microscope must be particularly on their guard against illusions of this kind. Raspail observes* that the hollow pyramidal arrangement of the crystals of muriate of soda appears, when seen through a microscope, like a striated pyramid in relief. He recommends two modes of correcting the illusion. The first is to bring successively to the focus of the instrument the different parts of the crystal; if the pyramid be in relief, the point will arrive at the focus sooner than the base will; if the pyramid be hollow, the contrary will take place. The second mode is to project a strong light on the pyramid in the field of view of the microscope, and to observe which sides of the crystal are illuminated, taking however the inversion of the image into consideration if a compound microscope be employed.

The inversion of relief is very striking when a skeleton cube is looked at with one eye, and the following singular results may in this case be observed. So long as the mind perceives the cube, however the figure be turned about, its various appearances will be but different representations of the same object, and the same primitive form will be suggested to the mind by all of them: but it is not so if the converse figure fixes the attention; the series of successive projections cannot then be referred to any figure to which they are all common, and the skeleton figure will appear to be continually undergoing a change of shape.

§ 12.

I have given ample proof that objects whose pictures do not fall on corresponding points of the two retinæ may still appear single. I will now adduce an experiment which proves that similar pictures falling on corresponding points of the two retinæ may appear double and in different places.

Present, in the stereoscope, to the right eye a vertical line, and to the left eye a

* Nouveau Système de Chimie Organique, 2me edit. t. 1. p. 333.

line inclined some degrees from the perpendicular (fig. 23.); the observer will then perceive, as formerly explained, a line, the extremities of which appear at different distances before the eyes. Draw on the left hand figure a faint vertical line exactly corresponding in position and length to that presented to the right eye, and let the two lines of this left hand figure intersect each other at their centres. Looking now at these two drawings in the stereoscope, the two strong lines, each seen by a different eye, will coincide, and the resultant perspective line will appear to occupy the same place as before; but the faint line which now falls on a line of the left retina, which corresponds with the line of the right retina on which one of the coinciding strong lines, viz. the vertical one, falls, appears in a different place. The place this faint line apparently occupies is the intersection of that plane of visual direction of the left eye in which it is situated, with the plane of visual direction of the right eye, which contains the strong vertical line.

This experiment affords another proof that there is no necessary physiological connection between the corresponding points of the two retinæ,—a doctrine which has been maintained by so many authors.

§ 13. *Binocular Vision of Images of different Magnitudes.*

We will now inquire what effect results from presenting similar images, differing only in magnitude, to analogous parts of the two retinæ. For this purpose two squares or circles, differing obviously but not extravagantly in size, may be drawn on two separate pieces of paper, and placed in the stereoscope so that the reflected image of each shall be equally distant from the eye by which it is regarded. It will then be seen that, notwithstanding this difference, they coalesce and occasion a single resultant perception. The limit of the difference of size within which the single appearance subsists may be ascertained by employing two images of equal magnitude, and causing one of them to recede from the eye while the other remains at a constant distance; this is effected merely by pulling out the sliding board C (fig. 8.) while the other C′ remains fixed, the screw having previously been removed.

Though the single appearance of two images of different size is by this experiment demonstrated, the observer is unable to perceive what difference exists between the apparent magnitude of the binocular image and that of the two monocular images; to determine this point the stereoscope must be dispensed with, and the experiment so arranged that all three shall be simultaneously seen; which may be done in the following manner:—The two drawings being placed side by side on a plane before the eyes, the optic axes must be made to converge to a nearer point as at fig. 4., or to a more distant one as at fig. 3., until the three images are seen at the same time, the binocular image in the middle, and the monocular images at each side. It will thus be seen that the binocular image is apparently intermediate in size between the two monocular ones.

If the pictures be too unequal in magnitude, the binocular coincidence does not

take place. It appears that if the inequality of the pictures be greater than the difference which exists between the two projections of the same object when seen in the most oblique position of the eyes (*i. e.* both turned to the extreme right or to the extreme left), ordinarily employed, they do not coalesce. Were it not for the binocular coincidence of two images of different magnitude, objects would appear single only when the optic axes converge immediately forwards; for it is only when the converging visual lines form equal angles with the visual base (the line joining the centres of the two eyes) as at fig. 2., that the two pictures can be of equal magnitude; but when they form different angles with it, as at fig. 24., the distance from the object to each eye is different, and consequently the picture projected on each retina has a different magnitude. If a piece of money be held in the position *a*, (fig. 24.) while the optic axes converge to a nearer point *c*, it will appear double, and that seen by the left eye will be evidently smaller than the other.

§ 14. *Phenomena which are observed when objects of different forms are simultaneously presented to corresponding parts of the two retinæ.*

If we regard a picture with the right eye alone for a considerable length of time it will be constantly perceived; if we look at another and dissimilar picture with the left eye alone its effect will be equally permanent; it might therefore be expected, that if each of these pictures were presented to its corresponding eye at the same time the two would appear permanently superposed on each other. This, however, contrary to expectation, is not the case.

If *a* and *b* (fig. 25.) are each presented at the same time to a different eye, the common border will remain constant, while the letter within it will change alternately from that which would be perceived by the right eye alone to that which would be perceived by the left eye alone. At the moment of change the letter which has just been seen breaks into fragments, while fragments of the letter which is about to appear mingle with them, and are immediately after replaced by the entire letter. It does not appear to be in the power of the will to determine the appearance of either of the letters, but the duration of the appearance seems to depend on causes which are under our control: thus if the two pictures be equally illuminated, the alternations appear in general of equal duration; but if one picture be more illuminated than the other, that which is less so will be perceived during a shorter time. I have generally made this experiment with the apparatus, fig. 6. When complex pictures are employed in the stereoscope, various parts of them alternate differently.

There are some facts intimately connected with the subject of the present article which have already been frequently observed. I allude to the experiments, first made by Du Tour, in which two different colours are presented to corresponding parts of the two retinæ. If a blue disc be presented to the right eye and a yellow disc to the corresponding part of the left eye, instead of a green disc which would appear if these

two colours had mingled before their arrival at a single eye, the mind will perceive the two colours distinctly one or the other alternately predominating either partially or wholly over the disc. In the same manner the mind perceives no trace of violet when red is presented to one eye and blue to the other, nor any vestige of orange when red and yellow are separately presented in a similar manner. These experiments may be conveniently repeated by placing the coloured discs in the stereoscope, but they have been most usually made by looking at a white object through differently coloured glasses, one applied to each eye.

In some authors we find it stated, contrary to fact, that if similar objects of different colour be presented one to each eye, the appearance will be that compounded of the two colours. Dr. Reid* and Janin are among the writers who have fallen into this inconsiderate error, which arose no doubt from their deciding according to previous notions, instead of ascertaining by experiment what actually does happen.

§ 15.

No question relating to vision has been so much debated as the cause of the single appearance of objects seen by both eyes. I shall in the present section give a slight review of the various theories which have been advanced by philosophers to account for this phenomenon, in order that the remarks I have to make in the succeeding section may be properly understood.

The law of visible direction for monocular vision has been variously stated by different optical writers. Some have maintained with Drs. Reid and Porterfield, that every external point is seen in the direction of a line passing from its picture on the retina through the centre of the eye; while others have supposed with Dr. Smith that the visible direction of an object coincides with the visual ray, or the principal ray of the pencil which flows from it to the eye. D'Alembert, furnished with imperfect data respecting the refractive densities of the humours of the eye, calculated that the apparent magnitudes of objects would differ widely on the two suppositions, and concluded that the visible point of an object was not seen in either of these directions, but sensibly in the direction of a line joining the point itself and its image on the retina; but he acknowledged that he could assign no reason for this law. Sir David Brewster, provided with more accurate data, has shown that these three lines so nearly coincide with each other, that "at an inclination of 30°, a line perpendicular to the point of impression on the retina passes through the common centre, and does not deviate from the real line of visible direction more than half a degree, a quantity too small to interfere with the purposes of vision." We may, therefore, assume in all our future reasonings the truth of the following definition given by this eminent philosopher:—"As the interior eye-ball is as nearly as possible a perfect sphere, lines perpendicular to the surface of the retina must all pass through one single point,

* Enquiry, Sect. xiii.

namely the centre of its spherical surface. This one point may be called the centre of visible direction, because every point of a visible object will be seen in the direction of a line drawn from this centre to the visible point."

It is obvious, that the result of any attempt to explain the single appearance of objects to both eyes, or, in other words, the law of visible direction for binocular vision, ought to contain nothing inconsistent with the law of visible direction for monocular vision.

It was the opinion of Aguilonius, that all objects seen at the same glance with both eyes appear to be in the plane of the horopter. The horopter he defines to be a line drawn through the point of intersection of the optic axes, and parallel to the line joining the centres of the two eyes; the plane of the horopter to be a plane passing through this line at right angles to that of the optic axes. All objects which are in this plane, must, according to him, appear single because the lines of direction in which any point of an object is seen coincide only in this plane and nowhere else; and as these lines can meet each other only in one point, it follows from the hypothesis, that all objects not in the plane of the horopter must appear double, because their lines of direction intersect each other, either before or after they pass through it. This opinion was also maintained by Dechales and Porterfield. That it is erroneous, I have given, I think, sufficient proof, in showing that, when the optic axes converge to any point, objects before or beyond the plane of the horopter are under certain circumstances equally seen single as those in that plane.

Dr. Wells's "new theory of visible direction" was a modification of the preceding hypothesis. This acute writer held with Aguilonius, that objects are seen single only when they are in the plane of the horopter, and consequently that they appear double when they are either before or beyond it; but he attempted to make this single appearance of objects only in the plane of the horopter to depend on other principles, from which he deduced, contrary to Aguilonius, that the objects which are doubled do not appear in the plane of the horopter, but in other places which are determined by these principles. Dr. Wells was led to his new theory by a fact which he accidentally observed, and which he could not reconcile with any existing theory of visible direction; this fact had, though he was unaware of it, been previously noticed by Dr. Smith; it is already mentioned in § 8., and is the only instance of binocular vision of relief which I have found recorded previous to my own investigations. So little does Dr. Wells's theory appear to have been understood, that no subsequent writer has attempted either to confirm or disprove his opinions. It would be useless here to discuss the principles of this theory, which was framed to account for an anomalous individual fact, since it is inconsistent with the general rules on which that fact has been now shown to depend. Notwithstanding these erroneous views, the "essay upon single vision with two eyes" contains many valuable experiments and remarks, the truth of which are independent of the theory they were intended to illustrate.

The theory which has obtained greatest currency is that which assumes that an object is seen single because its pictures fall on corresponding points of the two retinæ, that is on points which are similarly situated with respect to the two centres both in distance and position. This theory supposes that the pictures projected on the retinæ are exactly similar to each other, corresponding points of the two pictures falling on corresponding points of the two retinæ. Authors who agree with regard to this property, differ widely in explaining why objects are seen in the same place, or single, according to this law. Dr. Smith makes it to depend entirely on custom, and explains why the eyes are habitually directed towards an object so that its pictures fall on corresponding parts in the following manner:—"When we view an object steadily, we have acquired a habit of directing the optic axes to the point in view; because its pictures falling upon the middle points of the retinas, are then distincter than if they fell upon any other places; and since the pictures of the whole object are equal to one another, and are both inverted with respect to the optic axes, it follows that the pictures of any collateral point are painted upon corresponding points of the retinas."

Dr. Reid, after a long dissertation on the subject, concludes, "that by an original property of human eyes, objects painted upon the centres of the two retinæ, or upon points similarly situated with regard to the centres, appear in the same visible place; that the most plausible attempts to account for this property of the eyes have been unsuccessful; and therefore, that it must be either a primary law of our constitution, or the consequence of some more general law which is not yet discovered."

Other writers who have admitted this principle have regarded it as arising from anatomical structure and dependent on connexion of nervous fibres; among these stand the names of Galen, Dr. Briggs, Sir Isaac Newton, Rohault, Dr. Hartley, Dr. Wollaston and Professor Müller.

Many of the supporters of the theory of corresponding points have thought, or rather have admitted, *without thinking*, that it was not inconsistent with the law of Aguilonius; but very little reflection will show that both cannot be maintained together; for corresponding lines of visible direction, that is, lines terminating in corresponding points of the two retinæ, cannot meet in the plane of the horopter unless the optic axes be parallel, and the plane be at an infinite distance before the eyes. Some of the modern German writers* have inquired what is the curve in which objects appear single while the optic axes are directed to a given point, on the hypothesis that objects are seen single only when they fall on corresponding points of the two retinæ. An elegant proposition has resulted from their investigations, which I shall need no apology for introducing in this place, since it has not yet been mentioned in any English work.

R and L (fig. 26.) are the two eyes; C A, C′ A the optic axes converging to the

* *Tortual*, die Sinne des Menschen. Münster, 1827. *Bartels*, Beitrage zur Physiologie der Gesichtssinnes. Berlin, 1834.

point A; and C A B C′ is a circle drawn through the point of convergence A and the centres of visible direction C C′. If any point be taken in the circumference of this circle, and lines be drawn from it through the centres of the two eyes C C′, these lines will fall on corresponding points of the two retinæ D D′; for the angles A C B, A C′ B being equal, the angles D C E, D C′ E are also equal; therefore any point placed in the circumference of the circle C A B C′ will, according to the hypothesis, appear single while the optic axes are directed to A, or any other part in it.

I will mention two other properties of this binocular circle: 1st. The arc subtended by two points on its circumference contains double the number of degrees of the arc subtended by the pictures of these points on either retina, so that objects which occupy 180° of the supposed circle of single vision are painted on a portion of the retina extended over 90° only; for the angle D C E or D C′ E being at the centre, and the angle B C A or B C′ A at the circumference of a circle, this consequence follows. 2ndly. To whatever point of the circumference of the circle the optic axes be made to converge, they will form the same angle with each other; for the angles C A C′, C B C are equal.

In the eye itself, the centre of visible direction, or the point at which the principal rays cross each other, is, according to Dr. Young and other eminent optical writers, at the same time the centre of the spherical surface of the retina, and that of the lesser spherical surface of the cornea; in the diagram (fig. 26.), to simplify the consideration of the problem, R and L represent only the circle of curvature of the bottom of the retina, but the reasoning is equally true in both cases.

The same reasons, founded on the experiments in this memoir, which disprove the theory of Aguilonius, induce me to reject the law of corresponding points as an accurate expression of the phenomena of single vision. According to the former, objects can appear single only in the plane of the horopter; according to the latter, only when they are in the circle of single vision; both positions are inconsistent with the binocular vision of objects in relief, the points of which they consist appearing single though they are at different distances before the eyes. I have already proved that the assumption made by all the maintainers of the theory of corresponding points, namely that the two pictures projected by any object in the retinæ are exactly similar, is quite contrary to fact in every case except that in which the optic axes are parallel.

Gassendus, Porta, Tacquet and Gall maintained, that we see with only one eye at a time though both remain open, one according to them being relaxed and inattentive to objects while the other is upon the stretch. It is a sufficient refutation of this hypothesis, that we see an object double when one of the optic axes is displaced either by squinting or by pressure on the eye-ball with the finger; if we saw with only one eye, one object only should under such circumstances be seen. Again, in many cases which I have already explained, the simultaneous affection of the two retinæ excites a different idea in the mind to that consequent on either of the single impressions, the latter giving rise to the idea of a representation on a plane surface,

the former to that of an object in relief; these things could not occur did we see with only one eye at a time.

Du Tour* held that though we might occasionally see at the same time with both eyes, yet the mind cannot be affected simultaneously by two corresponding points of the two images. He was led to this opinion by the curious facts alluded to in § 14. It would be difficult to disprove this conjecture by experiment; but all that the experiments adduced in its favour, and others relating to the disappearance of objects to one eye really proves, is, that the mind is inattentive to impressions made on one retina when it cannot combine the impressions on the two retinæ together so as to resemble the perception of some external objects; but they afford no ground whatever for supposing that the mind cannot under any circumstances attend to impressions made simultaneously on points of the two retinæ, when they harmonize with each other in suggesting to the mind the same idea.

A perfectly original theory has been recently advanced by M. Lehot†, who has endeavoured to prove, that instead of pictures on the retinæ, images of three dimensions are formed in the vitreous humour which we perceive by means of nervous filaments extended thence from the retina. This theory would account for the single appearance to both eyes of objects in relief, but it would be quite insufficient to explain why we perceive an object of three dimensions when two pictures of it are presented to the eyes; according to it, also, no difference should be perceived in the relief of objects when seen by one or both eyes, which is contrary to what really happens. The proofs, besides, that we perceive external objects by means of pictures on the retinæ are so numerous and convincing, that a contrary conjecture cannot be entertained for a moment. On this account it will suffice merely to mention two other theories which place the seat of vision in the vitreous humour. Vallee‡, without denying the existence of pictures on the retina, has advocated that we see the relief of objects by means of anterior foci on the hyaloid membrane; and Raspail§ has developed at considerable length the strange hypothesis, that images are neither formed in the vitreous humour nor painted on the retina, but are immediately perceived at the focus of the lenticular system of which the eye is formed.

§ 16.

It now remains to examine *why* two dissimilar pictures projected on the two retinæ give rise to the perception of an object in relief. I will not attempt at present to give the complete solution of this question, which is far from being so easy as at a first glance it may appear to be, and is indeed one of great complexity. I shall in this place merely consider the most obvious explanations which might be offered, and show their insufficiency to explain the whole of the phenomena.

* Act. Par. 1743. M. p. 334.

† Nouvelle Théorie de la Vision, Par. 1823.

‡ Traité de la Science du Dessein, Par. 1821, p. 270.

§ Nouveau Système de Chimie Organique, t. 2. p. 329.

It may be supposed, that we see but one point of a field of view distinctly at the same instant, the one namely to which the optic axes are directed, while all other points are seen so indistinctly, that the mind does not recognize them to be either single or double, and that the figure is appreciated by successively directing the point of convergence of the optic axes successively to a sufficient number of its points to enable us to judge accurately of its form.

That there is a degree of indistinctness in those parts of the field of view to which the eyes are not immediately directed, and which increases with the distance from that point, cannot be doubted, and it is also true that the objects thus obscurely seen are frequently doubled. In ordinary vision, it may be said, this indistinctness and duplicity is not attended to, because the eyes shifting continually from point to point, every part of the object is successively rendered distinct; and the perception of the object is not the consequence of a single glance, during which only a small part of it is seen distinctly; but is formed from a comparison of all the pictures successively seen while the eyes were changing from one point of the object to another.

All this is in some degree true; but were it entirely so, no appearance of relief should present itself when the eyes remain intently fixed on one point of a binocular image in the stereoscope. But on performing the experiment carefully, it will be found, provided the pictures do not extend too far beyond the centres of distinct vision, that the image is still seen single and in relief when this condition is fulfilled. Were the theory of corresponding points true, the appearance should be that of the superposition of the two drawings, to which however it has not the slightest similitude. The following experiments are equally decisive against this theory.

Exp. 1. Draw two lines about two inches long and inclined towards each other, as in fig. 10., on a sheet of paper, and having caused them to coincide by converging the optic axes to a point nearer than the paper, look intently on the upper end of the resultant line, without allowing the eyes to wander from it for a moment. The entire line will appear single and in its proper relief, and a pin or a piece of straight wire may without the least difficulty be made to coincide exactly in position with it; or, if while the optic axes continue to be directed to the upper and nearer end, the point of a pin be made to coincide with the lower and further end or with any intermediate point of the resultant line, the coincidence will remain exactly the same when the optic axes are moved and meet there. The eyes sometimes become fatigued, which causes the line to appear double at those parts to which the optic axes are not fixed, but in such case all appearance of relief vanishes. The same experiment may be tried with more complex figures, but the pictures should not extend too far beyond the centres of the retinæ.

Another and a beautiful proof that the appearance of relief in binocular vision is an effect independent of the motions of the eyes, may be obtained by impressing on the retinæ ocular spectra of the component figures. For this purpose the drawings should be formed of broad coloured lines on a ground of the complementary colour,

for instance red lines on a green ground, and be viewed either in the stereoscope or in the apparatus, fig. 6., as the ordinary figures are, taking care however to fix the eyes only to a single point of the compound figure; the drawings must be strongly illuminated, and after a sufficient time has elapsed to impress the spectra on the retinæ, the eyes must be carefully covered to exclude all external light. A spectrum of the object in relief will then appear before the closed eyes. It is well known, that a spectrum impressed on a single eye and seen in the dark, frequently alternately appears and disappears: these alternations do not correspond in the spectra impressed on the two retinæ, and hence a curious effect arises; sometimes the right eye spectrum will be seen alone, sometimes that of the left eye, and at those moments when the two appear together, the binocular spectrum will present itself in bold relief. As in this case the pictures cannot shift their places on the retinæ in whatever manner the eyes be moved about, the optic axes can during the experiment only correspond with a single point of each.

When an object, or a part of an object, thus appears in relief while the optic axes are directed to a single binocular point, it is easy to see that each point of the figure that appears single is seen at the intersection of the two lines of visible direction in which it is seen by each eye separately, whether these lines of visible direction terminate at corresponding points of the two retinæ or not.

But if we were to infer the converse of this, viz. that every point of an object in relief is seen by a single glance at the intersection of the lines of visible direction in which it is seen by each eye singly, we should be in error. On this supposition, objects before or beyond the intersection of the optic axes should never appear double, and we have abundant evidence that they do. The determination of the points which shall appear single seems to depend in no small degree on previous knowledge of the form we are regarding. No doubt, some law or rule of vision may be discovered which shall include all the circumstances under which single vision by means of non-corresponding points occurs and is limited. I have made numerous experiments for the purpose of attaining this end, and have ascertained some of the conditions on which single and double vision depend, the consideration of which however must at present be deferred.

Sufficient, however, has been shown to prove that the laws of binocular visible direction hitherto laid down are too restricted to be true. The law of Aguilonius assumes that objects in the plane of the horopter are alone seen single; and the law of corresponding points carried to its necessary consequences, though these consequences were unforeseen by its first advocates, many of whom thought that it was consistent with the law of Aguilonius, leads to the conclusion, that no object appears single unless it is seen in a circle passing through the centres of visible direction in each eye and the point of convergence of the optic axes. Both of these are inconsistent with the single vision of objects whose points lie out of the plane in one case and the circle in the other; and that objects do appear single under circumstances

that cannot be explained by these laws, has, I think, been placed beyond doubt by the experiments I have brought forward. Should it be hereafter proved, that all points in the plane or in the circle above mentioned are seen single, and from the great indistinctness of lateral images it will be difficult to give this proof, the law must be qualified by the admission, that points out of them do not always appear double.

XIX. *An Experimental Inquiry into the influence of Nitrogen on the growth of Plants.* By Robert Rigg. *Communicated by the Rev.* J. B. Reade, *M.A. F.R.S.*

Received May 10,—Read May 31, 1838.

About two years ago I had the honour of laying before the Royal Society an experimental inquiry into some of the chemical changes which occur during the germination of seeds and the decomposition of vegetable matter. On the present occasion I purpose to confine myself to an extensive series of experiments which have reference to the presence of nitrogen, earths, and salts in vegetable compounds, with a view of directing attention to the influence of nitrogen in the growth of vegetables.

As my inquiry is purely experimental, I may premise that I have had recourse to the well-known method of ultimate analysis, and the equivalent numbers which I employ are, carbon 6·12, hydrogen 1·0, oxygen 8·0, and nitrogen 14·0. That we may the more readily apply the proportionate quantity of nitrogen to our immediate purpose, I shall make one column in each analysis, which will represent by weight the quantity of nitrogen when compared with 1000 parts of carbon in the same compound. I also designate by the term *residual* those earthy and saline ingredients which are not decomposed during the analysis. In some of the experiments this residual may contain a little *foreign matter*, for in preparing the different compounds for analysis I seldom had recourse to any process of ablution, rather choosing to have a little foreign matter present, than to remove any part of that which was more particularly the object of research. That I might also examine the compounds as nearly as possible in their natural state, I very rarely exposed them to a higher temperature than 100° Fahr., inclosing them in very thin paper, and afterwards allowing them to acquire the hygrometric state of the atmosphere.

The first series of experiments to which I shall refer tends to show, that in that part of the seed where germination takes place nitrogen preponderates, when compared with its quantity in the other part of the seed. This result is derived from the analysis of the germ and cotyledons of beans, peas, barley, wheat, &c., a large excess of nitrogen being invariably indicated in the germ.

Table I.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Germ of the garden bean	42·68	1·19	....	8·53	1·80	45·8 = 100	200
Cotyledons of the garden bean	39·27	....	2·66	5·65	2·40	50·02 = 100	140
Germ of early garden peas	41·9	0·2	....	8·3	0·8	48·8 = 100	198
Cotyledons of early garden peas	40·1	....	6·5	4·2	1·3	47·9 = 100	104
Germinating ends of barley	39·6	0·2	....	1·9	0·6	57·7 = 100	48
The other parts of barley	39·2	....	....	1·0	0·8	59·0 = 100	25
Germinating ends of wheat	41·2	0·9	....	2·1	0·7	55·1 = 100	51
The other parts of wheat	40·6	0·3	....	1·6	0·8	56·7 = 100	39

Thus, for instance, it appears from the table of analysis, that the germ of beans and peas contain by weight about 200 parts of nitrogen for 1000 parts of carbon, while the cotyledons contain only from about 100 to 140 parts.

A second series of experiments disposes me to think, that those *seeds of the same kind* which contain the largest quantity of nitrogen germinate the earliest. Barley of the growth of 1835, containing 46 parts of nitrogen for 1000 of carbon, germinated in thirty-six hours after being taken out of the water in which it had been steeped; whereas barley of 1837, and containing only 35 parts of nitrogen, steeped in water at the same time, and kept under the same circumstances as the former, germinated in forty-eight hours. Similarly, I find that of the seeds, mustard, cress, rape, turnip, radish, and celery, those which contain the largest quantity of nitrogen and residual, germinate the earliest when kept under equal circumstances. It is necessary to state, that in these analyses the seeds were examined in the mass.

TABLE II.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Barley of 1835	43·93	0·71	....	2·02	1·30	52·04 = 100	46
Barley of 1837	39·57	....	3·45	1·38	1·30	54·30 = 100	35
Mustard seed	50·74	2·36	....	3·55	3·90	39·45 = 100	70
Cress seed	46·8	1·5	....	3·3	4·8	43·6 = 100	71
Rape seed	55·3	3·4	....	2·7	3·1	35·5 = 100	50
Turnip seed	55·4	3·5	....	3·6	3·1	34·4 = 100	65
Radish	55·34	3·48	....	5·03	3·4	32·75 = 100	90
Celery	50·39	2·35	....	2·37	6·6	38·29 = 100	47

The chemical constitution of the *rootlets of seeds* before the *plumula* extends the whole length of the seeds, as in the instance of malted barley, differs from that of the malt, and also from the constitution of the barley in its original state. In these we have the rootlets containing a large quantity of nitrogen at a period when they will have to perform important offices in preparing the food for the young plant. That there is a similar difference between the chemical constitution of the roots and trunks of trees will abundantly appear from the annexed Table. And I may also add, that my experiments dispose me to infer that the quantity of nitrogen is largest in the spring, and diminishes with the season.

TABLE III.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Barley in its original state	39·6	....	3·5	1·3	1·3	54·3 = 100	32
The malt made from the same	41·7	....	1·8	2·1	1·4	53·0 = 100	50
The rootlets of the malted barley	40·5	0·4	....	4·3	3·5	51·3 = 100	106
Root of an apple-tree with the bark	41·6	....	3·7	·7	1·2	52·8 = 100	16
Trunk of an apple-tree without the bark	42·8	....	4·8	·3	·3	51·8 = 100	7
Root of a plum-tree with the bark	42·4	0·4	....	·8	1·6	54·8 = 100	18
Trunk of the same without the bark	42·8	....	....	·4	0·6	56·2 = 100	9
Root of cherry-tree with the bark	45·5	0·2	....	1·1	·8	52·4 = 100	18
Trunk of cherry-tree without the bark	43·9	....	4·3	·6	·2	51·0 = 100	10
Ash in a very dry state without the bark	46·6	....	....	0·5	·8	52·1 = 100	11
Ash root without the bark	35·2	0·8	....	2·6	9·4	52·0 = 100	74

Note.—The apple, plum, and cherry-trees were all of them very small; they had been in the ground several years, and had been rooted up because of their general unhealthiness. In a healthy state of the trees the nitrogen of the root is in a larger proportion.

But not only is the nitrogen more abundant in the roots of plants and trees; the residual also, when compared with the quantity in the trunks, will be found in excess in the roots.

Now if we admit the principle, that nitrogen is a powerful agent in favouring chemical action upon vegetable and animal matter, and that this residual is essential to the healthy performance of every function of the roots, as well as every other part of the plant, and forms, as it were, a most perfect skeleton of the whole; we have in these roots that which will favour such action in an eminent degree when compared with the other part of the tree.

It would be leading us into other subjects more extensive than the one now before us, if I were to go into, or treat upon, the chemical action which takes place by the agency of the roots, the compounds formed thereby, the heat produced by such action, the arrangement of the residual, &c. It will be sufficient, that in following up this part of the inquiry, we state as the result of experiment, that the *sap wood* is very differently constituted from the more perfect part, *the heart wood*, an excess of nitrogen being invariably found in the former.

TABLE IV.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Young English oak sap wood	42·40	·18	....	·55	1·6	55·27 = 100	13
Heart wood of ditto	42·40	....	....	·16	0·7	56·74 = 100	4
Quebec oak sap wood	44·2	·14	....	·31	1·3	54·05 = 100	7
Heart wood of ditto	44·59	....	....	·13	0·6	54·68 = 100	3
English elm sap wood	41·20	·40	....	1·6	2·5	54·3 = 100	39
Heart wood of ditto	41·1	....	....	0·7	1·7	56·5 = 100	17
Acacia sap wood	43·79	....	2·49	·55	·50	52·67 = 100	13
Heart wood of ditto	41·36	....	6·67	·50	·20	51·27 = 100	12
Cedar from Africa sap wood	42·06	·42	....	·39	2·8	54·33 = 100	10
Heart wood of ditto	39·92	....	....	·36	0·7	59·02 = 100	9
Chestnut sap wood	41·16	....	1·86	·38	·40	56·20 = 100	9
Heart wood of ditto	40·18	.....	7·96	·29	·20	51·37 = 100	7

It will be unnecessary for me to say that the sap wood more readily passes into a state of decay than the heart wood. Here again the nitrogen and the residual being present in larger quantities in the former than in the latter, we have them exerting their influence as promoters of decomposition.

We have also the greatest quantities of nitrogen and residual in those timbers which grow the quickest: and further than this; for directly as the quantity of nitrogen and residual taken collectively, so do we appear to have the decay of timber, all other circumstances being equal. The following is the analysis of several kinds of timber which favour this inference.

TABLE V.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
Satin wood	47·20	....	....	·04	·50	52·26	= 100	1
Dantzic oak	44·90	....	....	·11	·50	54·49	= 100	2
English oak	43·20	....	....	·20	·60	56·00	= 100	4
Malabar teak	46·82	·48	....	·26	·18	52·26	= 100	5
Rangoon teak	47·93	·34	....	·43	·22	51·08	= 100	9
Spanish mahogany, fine-grained	49·0	....	....	·10	·50	50·4	= 100	2
Honduras mahogany, coarse-grained	40·97	....	....	·90	1·50	56·63	= 100	23
Lignum vitæ	51·22	1·21	....	·56	·60	46·41	= 100	11
Box	46·4	·50	....	·50	·80	51·80	= 100	11
Rose wood	51·1	....	....	·60	2·50	45·8	= 100	12
Black ebony	42·4	....	....	1·50	5·0	51·1	= 100	35
American birch	45·0	....	....	2·2	1·5	51·3	= 100	40

Thus, for instance, the nitrogen in the satin wood may be considered almost inappreciable; and the same may be said of the residual in the Malabar teak, the nitrogen being also small in this timber. In Dantzic and English oak the quantity of nitrogen and residual are both very small. In American birch the nitrogen and residual are in large quantities, and, as is well known, this timber decays very quickly.

But it is not enough for us to find a difference in the proportionate quantity of nitrogen in the different parts of the same plant or tree; we must also observe that the quantity appears to be proportional to the functions which the parts of the plants have to perform in vegetation. For instance, if the agency of any part of the plant be great in the scale of vegetable physiology, so is the quantity of nitrogen, and *vice versâ*. So apparent is this, and so universal is the operation of this law over the whole sphere of inquiry in which I have been engaged, that we might almost consider this element, when coupled with the residual, to be the moving agent, acting under the influence of the living principle of the plant, and moulding into shape the other elements. We have this beautifully instanced in the chemical constitution of the different parts of wheat, barley, oats, common grass, turnips, cabbages, carrots, potatoes, &c., found by subjecting their various parts to analysis *at different periods of their growth* (See Table VI.). For by thus subjecting the different parts of the same plant to analysis at different periods of growth, we acquire much valuable information upon vegetation generally, and respecting the influence of nitrogen and residual in particular.

TABLE VI.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
Flour of wheat not nearly ripe	41·2	1·8	....	2·9	2·0	52·1	= 100	70
Flour of the same kind nearly ripe	40·6	0·5	....	2·3	1·0	55·6	= 100	57
Leaves of the wheat not nearly ripe	37·6	....	8·1	3·3	4·2	46·8	= 100	87
Leaves of the same when nearly ripe	38·4	....	....	2·1	4·6	54·9	= 100	55
Stems of the wheat not nearly ripe	39·8	0·8	....	3·5	4·0	51·9	= 100	87
Stems of the same when nearly ripe	38·8	....	....	1·3	4·0	55·9	= 100	33
Chaff of wheat not nearly ripe	35·5	....	7·3	1·8	10·8	44·6	= 100	50
Chaff of the same when nearly ripe	31·2	....	1·7	1·3	11·0	54·8	= 100	42
Common grass not growing freely	41·1	....	3·1	4·4	5·5	45·9	= 100	107
Common grass gathered at the same time, growing very freely	39·5	1·6	....	5·6	6·5	46·8	= 100	141
Turnip when attacked by the fly	35·5	....	....	8·0	13·4	43·1	= 100	224
Cabbage leaf not eaten	39·5	....	4·8	8·1	5·9	41·7	= 100	203
The part eaten by insects	39·7	....	13·8	5·7	8·0	32·8	= 100	143
The insects themselves	36·0	1·3	....	6·3	14·0	42·4	= 100	175
Green part of another cabbage leaf	39·9	....	3·8	6·5	4·7	45·1	= 100	162
White part of the same	39·2	0·8	....	8·0	4·9	47·1	= 100	205
Tendril of the same	38·8	2·7	....	5·4	6·3	46·8	= 100	138
Very centre part of the cabbage	33·0	1·7	....	4·1	4·0	57·2	= 100	124
Root of the same plant	39·2	1·4	....	5·5	4·5	49·4	= 100	141
Red clover stems	29·6	0·4	....	2·5	9·8	57·7	= 100	83
Leaf	28·6	....	7·7	4·2	5·0	54·5	= 100	145
Flower	30·4	....	10·2	3·6	5·0	50·8	= 100	119
Potato itself	37·1	1·4	....	2·9	3·4	55·2	= 100	79
Stem of the same	25·3	....	18·1	3·1	15·0	38·5	= 100	123
Leaves of the same	39·8	0·5	....	8·5	9·4	41·8	= 100	214
Apple of the same	32·9	....	16·4	3·9	5·6	41·2	= 100	117
Corolla of the same	38·8	....	8·5	3·3	4·4	45·0	= 100	85
Pistils of the same	36·2	2·2	....	4·6	9·6	47·4	= 100	129
Young carrot, $\frac{1}{4}$ of an inch in diameter	33·1	1·5	....	2·9	8·5	54·0	= 100	88
Leaves of the same	30·4	0·8	....	2·7	10·0	56·1	= 100	90
Stems of the same	28·7	....	2·8	1·7	11·2	55·6	= 100	59

There appear indeed to be various chemical actions taking place, in which these two elements are eminently concerned, viz. in the preparation of the food of the plants by the roots, and in combining this food with the other elements and fitting the whole to the various purposes of the plants.

Throughout the whole course of my experimental inquiry, I have not met with one instance wherein we have a large proportion of nitrogen and residual, that we have not violent chemical action and quick growth of the plants, all other circumstances being favourable.

By analysing *the leaves of trees* we may throw further light upon the operation of nitrogen. Of the almost numberless vegetables which cover the face of the earth, there are very few, if any, whose growth and produce afford us more information upon the chemical changes which occur during the growth of plants and the decomposition of vegetable matter than the vine. Its abundant flow of sap in the spring yields us a most important product for determining its food. Its foliage furnishes us with a plentiful supply of leaves for examination at different seasons: and by allow-

ing these leaves sometimes to remain on the trees until they are very abundant, and then removing a considerable portion thereof, leaving the rest to grow, we have at intervals of very few days an opportunity of chemically examining this very important and indispensable part of vegetable production under very different circumstances. By carefully dissecting these leaves, we are enabled to discover by analysis important changes produced in very few hours. From the proneness on the part of these leaves to pass into decomposition, at favourable temperatures, we have a feature brought before us which claims our best attention. And we have the fruit of this plant affording us, in its conversion into wine and other substances, an opportunity of examining into many important chemical changes, and I may add, of making the accuracy of many popular theories more than questionable.

The vines which more generally afforded me materials for examination are those which produce the white and black sweet-water grapes. They are in the open air, and are nailed to the south side of a brick wall. A series of experiments upon the leaves of these vines are given in Table VII., showing in a striking manner that nitrogen is in large quantities when they first make their appearance; that as they are developed, it decreases in proportionate quantity; that it is in excess during the period of their most rapid growth; and that towards the close of the year it is comparatively small.

TABLE VII.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
The first small leaves on the white grape vine	37·7	0·5	....	5·1	9·5	47·2 = 100	134
Leaves from the same about a month afterwards	42·2	0·4	....	5·3	3·8	48·3 = 100	126
Leaves from the same in July	39·8	....	4·2	3·5	3·8	48·7 = 100	88
Leaves from the same in August	39·1	....	6·1	2·9	6·6	45·3 = 100	74
Leaves from the same in November	41·9	....	....	2·3	9·2	46·6 = 100	55
The first leaves on the black grape vine	41·8	1·4	....	7·8	10·3	38·7 = 100	185
Leaves from the same in June	42·8	....	3·8	5·4	3·8	44·2 = 100	126
Leaves from the same in July	41·5	....	1·1	3·6	3·0	50·8 = 100	88

With a view of ascertaining whether or not these peculiarities in the chemical constitution of the leaves of plants and trees were universal, I have had recourse to extensive analyses thereof, gathering the leaves from a great number of trees at different stages of their growth. The results hereby furnished may be obtained from the experiments in Table VIII.

TABLE VIII.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
First small leaves from the lime tree in May	41·9	1·0	....	7·1	6·5	43·5 = 100	169
From the same in September 17	34·3	....	11·3	3·6	5·4	45·4 = 100	105
From the same in October 12	33·8	....	3·4	2·9	5·6	54·3 = 100	76
Acacia leaves, August 26, 1836	43·3	0·4	....	6·2	4·4	45·7 = 100	144
Acacia leaves, October 20, 1836	35·8	....	....	3·3	6·4	50·5 = 100	82
Almond leaves, August 26, 1836	37·5	....	11·7	4·4	3·8	42·6 = 100	118
Almond leaves, September 27	37·0	....	....	3·8	4·0	55·2 = 100	102
Plane tree leaves, September 26, 1836	41·4	....	18·7	2·7	4·4	32·8 = 100	65
Plane tree leaves, October 26	45·3	....	....	2·4	3·8	48·5 = 100	53
Red currant, May 3	40·1	....	2·0	6·2	3·5	48·2 = 100	155
Red currant, August 25	44·6	....	....	5·7	3·6	46·1 = 100	129
Very young ivy	40·4	1·6	....	3·6	4·6	49·8 = 100	90
Full grown ivy	41·6	0·4	....	3·2	5·2	49·6 = 100	78
Decaying ivy	42·4	....	....	2·2	5·8	49·6 = 100	52
Oak leaves, July 1836	40·8	0·6	....	4·3	3·9	50·4 = 100	104
Oak leaves, August	38·4	....	....	3·8	4·0	53·8 = 100	100

The analyses of the different parts of the flowers of plants are full of interest. The parts not only differ in chemical constitution with their state of developement, as appears in Table IX., in the instance of the rose, where the full-blown petals contained twenty-four parts of nitrogen, and the unexpanded and central petals contained sixty-six parts; but the various portions differ very materially from each other, and when taken in connexion with the germination of seeds, the growth of plants, their aliment, &c., throw much light upon the whole subject.

TABLE IX.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Full-blown rose petals	42·2	....	2·6	1·0	3·0	51·2 = 100	24
Rose petals not expanded, gathered at the same time, and from the same tree	45·2	1·1	....	3·0	2·5	48·2 = 100	66
Petals of the dahlia	35·5	....	10·2	2·7	4·0	47·6 = 100	77
Pistils of the same	34·8	1·3	....	4·2	4·3	55·4 = 100	120
Petals of the white lily	36·4	....	13·5	1·9	5·2	43·0 = 100	53
Pistils of the white lily	38·1	0·3	....	3·6	4·5	53·5 = 100	94
Pollen of the white lily	55·4	5·5	....	5·6	5·8	27·7 = 100	101
Stems of the anthers of white lily	40·5	....	....	2·2	5·0	52·3 = 100	55
Chrysanthemum, expanded petals	39·2	....	....	2·1	3·7	55·0 = 100	54
Chrysanthemum, unexpanded	39·2	3·0	....	2·9	2·4	52·5 = 100	74
Pollen of the same	43·2	1·6	....	3·0	1·8	50·4 = 100	69
Leaves of the same	40·2	0·6	....	2·8	8·2	48·2 = 100	70
Leaves gathered June 16	41·4	....	4·1	5·0	4·2	45·3 = 100	121

Without adding to the number of experiments already furnished, I would observe, that I have not analysed any product in a natural state wherein I have not found both nitrogen and residual; and, of the great number that I have subjected to this

process, those which are embodied in this paper may be considered as approximating to an average of the whole, as regards both this gaseous element and the incombustible matter.

In conclusion, I would observe that the mode of analysis which I have adopted in the examination of organic compounds, so far as determining the quantity of carbon, hydrogen, oxygen, and residual are concerned, is the one described in the paper on vegetable decomposition to which I have already referred. Respecting the mode of determining the quantity of nitrogen, a very brief account of the plan which I have adopted is given in the Philosophical Magazine for January last; and by combining these two methods of ultimate analysis, I am enabled, in recapitulation, to detect very minute errors, and therefore to speak with certainty as to the accuracy and value of every experiment.

XX. *On the Evolution of Nitrogen during the growth of Plants, and the Sources from whence they derive that element.* By Robert Rigg. *Communicated by the Rev.* J. B. Reade, *M.A. F.R.S.*

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In this communication I shall have the honour of submitting to the Royal Society a series of experiments, which not only confirm the principles I have already laid down in the investigations of the influence of nitrogen on the growth of plants, but also enable us to trace this important element to its source.

By what mysterious process of natural chemistry the living principle of the plant obtains this product, we must be content to remain ignorant; but to what extent a supply can be procured from the compounds upon which its agency is directed, is within the compass of legitimate experimental research.

With respect to the entire volume of nitrogen connected with the process of vegetation, it will be observed that my former Tables show that the quantity appropriated by the plant varies from $\frac{1}{1000}$ to $\frac{1}{4}$ of their weight of carbon; and the important experiments of Dr. Daubeny, Saussure, Sir Humphry Davy and others, have abundantly established the fact that there is a considerable evolution of nitrogen during the growth of plants. Experiments, however, upon the quantity of nitrogen evolved are so beset with difficulties, that our best efforts in this department of vegetable physiology have not enabled us to speak with certainty as to the absolute or relative quantities of the gases which are given off during the healthy action of the functions of vegetable life. And in corroboration of this remark it is only necessary to state, that the natural course of vegetation is unavoidably interrupted, by the necessity we are under of excluding plants from the free action of the external atmosphere, when we attempt to collect the gases they give off.

Sir Humphry Davy made his experiments under as favourable circumstances as the nature of the case will admit of; and he found that when the oxygen decreased in quantity about two per cent. the nitrogen increased four per cent., and that when the oxygen increased about thirty-five per cent. the nitrogen increased about seven per cent. Some of my own experiments have been conducted on the principle adopted by Sir Humphry Davy. I have also introduced whole branches of trees, as well as stems and leaves of more tender plants, under water, without removing them from the parent stock, and collected their gaseous products in inverted glass vessels. Upon examining these products, I find, that the gaseous volumes are perceptibly affected by the brightness of the sun's rays, though their variation appears to be subject to no

fixed law. At the same time, however, it is certain that nitrogen, whether appropriated or evolved, is apparently the most uniform, while oxygen assumes the character of a most fluctuating and passive agent.

Hence, then, if plants, taken as a whole, contain from three to four per cent. of their weight of nitrogen, and if to this we add the indefinite quantity, so far as we can comprehend, which is evolved during their growth, the question naturally arises, From whence do plants draw this part of their substance, as well as the volume of nitrogen which they furnish to the atmosphere? a question of the greatest importance in the practical application of the subject, and one not wanting in interest as a part of the inquiry into the chemical changes which occur during the growth of plants.

The quantity of animal and vegetable matter which forms a part of all soils suitable for the growth of plants, invariably containing nitrogen, furnishes us at once with one source from whence they may derive a part, if not the whole, of this element, which is either found in their constitution, or given off to the atmosphere during their growth. Hence an accurate examination of these soils at different periods might enable us to determine the question as to whether or not the earth supplied the whole of the nitrogen which is employed in vegetable economy. But from the many difficulties consequent upon accurately experimenting upon soils previous to and after the production of any plants, and also the examination of the plants so produced, we are not able by this mode of proceeding to obtain results in any degree satisfactory.

In order to free myself from all doubt upon this point, and to ascertain whether *the atmosphere* did or did not furnish any part, and if any what proportion, of the nitrogen which enters into the constitution of plants, I had recourse to the ultimate analysis of seeds and young seedling plants.

Some seeds I steeped in distilled water, and some in filtered Thames river water. When they had absorbed a sufficient quantity of water to favour the first impulse of germination, I spread them on glass or china plates, and kept them at temperatures favourable to germination and vegetation, in a room where the only source of nitrogen would be that which was supplied by the seed and the atmosphere. The quantity of dry seed experimented upon varied from thirty to one thousand grains, which were accurately weighed. These I allowed to germinate and grow, keeping them regularly supplied with the respective kinds of water only; and that nothing might be lost by the water carrying off any of the soluble parts of the different seeds, that water which was employed in steeping each kind of seed at the commencement was afterwards used in watering the same.

Some of the experiments I favoured so as to have them germinating and growing quickly, and others slowly. Some of them were terminated at one stage of their growth, and some at another; and some of them were allowed to vegetate as long as the seeds appeared to afford them nourishment. At the end of each experiment the whole mass was enveloped in very thin paper, and dried at a temperature varying from 100° to 110° Fahr., powdered, and afterwards allowed to acquire the hygrometric state

of the atmosphere. They were accurately weighed in this condition, a portion of each part, or of the entire mass, subjected to ultimate analysis, and calculation made for the whole. Thus I was enabled to discover an increase or decrease of any of the elements in any experiment so conducted, wherein the seed itself, the water, and the atmosphere furnished whatever was required for vegetation. The experiments which have reference to this part of the subject are found in Table X.

We are led by these experiments to the inference, that, independent of that indefinite quantity of nitrogen which is given off to the atmosphere during vegetation, there is an increase of this element in plants when compared with its quantity in the seeds; and in this case the seeds form the only source from whence they could derive it, with the exception of the atmosphere and any little which might have combined with the water used on the occasion.

It would be at variance with my mode of research, which is purely experimental, to make any observations upon the quantity of nitrogen which is probably furnished by the atmosphere during the germination of seeds and the full growth and developement of plants. The experiments before us dispose us to infer that it differs with the temperature at which the plants are exposed, and with exposure as regards sunshine and the shade. Thus we have in the germination and vegetation of barley, for instance, the quantity of nitrogen in the grain germinated under very favourable circumstances for the process, increasing to an extent equal to thirty-eight per cent. upon the original quantity contained in the seed: when the same kind of barley was kept under unfavourable circumstances for vegetation, and allowed to grow until the principal part of the farina was exhausted, the increase in the quantity of nitrogen was only eighteen per cent. When the same grain was allowed to vegetate in the sun's rays until about two-thirds of the flour contained in the seed had disappeared, the increase in the quantity of nitrogen was thirty per cent.; and when the same plants were kept under the most favourable circumstances, and allowed to vegetate until the seeds appeared to be exhausted, the young plant during this time having the most healthy appearance, there was an increase upon the quantity of nitrogen contained in the seeds of *nearly fifty per cent.*

Seeds of cress during vegetation increased their quantity of nitrogen forty-one per cent. when the plants were kept under a temperature varying with shade and sunshine from 60° to 84°. The experiments upon the seeds of the turnip, an important plant in an agricultural point of view, (about one half of which germinated,) shows that the nitrogen which was derived from the atmosphere was more than that which was contained in the seed. In all these experiments we have the quickness of the growth of the young plants proportional to the quantity of nitrogen present when compared with 1000 parts of carbon in the same.

With these facts before us, we are enabled to account for plants not continuing to grow so well in pure oxygen gas as in atmospheric air; and by following up the inquiry in other departments, we see the wisdom of the all-wise Contriver in consti-

tuting an atmosphere with a decided preponderance of nitrogen and a much smaller proportion of oxygen.

Finding that plants under different circumstances of growth differ in the relative quantity of nitrogen which they contain, as well as in the other elements which enter into their constitution, I was desirous of obtaining information which would lead us to account for the well-known fact of plants increasing most in size during cloudy weather, and of grasses, for instance, which are shaded (as under hedges), increasing much in straw, but producing seed both small in quantity and inferior in quality.

By way of commencement in this part of the research, I made duplicate experiments with the same kind of seeds, steeped them in and supplied them with the same kind of water, and kept them under equal circumstances in every respect, except that of placing one of each in the sun's rays in the greenhouse, where the sun shone till three P.M., and the other in the same situation, excluded however from the sun's rays, but not from the light. Those in the shade increased in length much more than the others, were a little lighter in colour, and when weighed before they were exposed to the drying temperature, were also heavier, but when dried at 100° to 110° were considerably less in weight. The ultimate analyses of these experiments are found in Table XI., and the result of the experiments as regards nitrogen is, that those plants which weighed the heaviest before drying, and which had as it were moulded into shape the largest quantity of matter in the form of plants, contained, in the cress for instance, 147 parts of nitrogen for every 1000 parts of carbon; whereas those plants from seed of the same kind which grew in the sun's rays, and whose weight before drying and when freed from foreign water was nine per cent. lighter than the other, contained only 111 parts of nitrogen for 1000 of carbon;—and in addition I might make an observation which is in perfect harmony with all that has been noticed upon the influence of nitrogen on the growth of plants, viz. that whereas the cress arrived at its state of maturity, so far as the seed could furnish it with nourishment, in eighteen days, the rape had not exhausted all its seed in twenty-six days; and the proportionate quantity of nitrogen in those plants was, in cress in the sun's rays 111, in rape in the same situation 73; and in cress in the shade 147, and in rape 82, when compared with 1000 parts by weight of carbon in each.

In concluding this subject, upon which I have been as brief as its nature would admit of, not even entering at all upon the practical application thereof, the point of view wherein its real value consists, I beg to observe that, although nitrogen appears from these experiments to be a very powerful agent in the economy of plants, it is far from my intention to give it any undue importance. It is my object to draw attention to an element which, comparatively speaking, has escaped unnoticed, and to vindicate the necessity of a most scrupulous attention to those products which, though so minute in quantity as to be with difficulty detected in our balances, have nevertheless been wisely assigned to discharge the most important functions.

TABLE X.

		Carb.	Hydr.	Oxyg.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Barley steeped in distilled water germinated quickly until the plumula of several seeds had passed through the grain.	The malt	35·14	....	1·51	1·73	1·15	44·67 = 84·2	49
	The rootlets and plumula	1·70	0·2	....	·18	·16	2·14 = 4·2	106
		36·84	0·2	1·51	1·91	1·31	46·58 = 88·4	
Barley in its original state		39·57	....	3·45	1·38	1·30	54·3 = 100	35
Increase		....	0·2	....	·53	·01		
Decrease		2·73	....	1·94	....	....	7·72 = 11·6	
Barley steeped in river water vegetated in the shade until the corn appeared to be exhausted.	The stems	7·98	·17	....	·61	·86	11·38 = 21·0	76
	The roots	8·90	....	....	·72	1·00	13·37 = 23·99	81
	The husks	6·19	....	·25	·26	·53	7·27 = 14·5	24
	The liquid separated by drying	·04	·01	....	·02	....	= ·07	
		23·11	·18	·25	1·61	2·39	32·02 = 59·56	
Barley in its original state		39·57	....	3·45	1·38	1·30	54·3 = 100	35
Increase		....	·18	....	·23	1·09		
Decrease		16·46	....	3·2	....	....	22·28 = 40·46	
Barley steeped in distilled water, and vegetated until about two-thirds of the flour had disappeared; kept in the sun's rays.	Original state	39·57	....	3·45	1·38	1·30	54·3 = 100	35
	The vegetated mass	29·7	·5	....	1·80	1·3	44·3 = 77·6	60
	Increase	....	·5	....	·42	....	.....	25
	Decrease	9·87	....	3·45	....	....	9·7 = 22·4	
Barley steeped in rain water grew until the stems were five and six inches long; kept in the sun's rays.	Original state	39·57	....	3·45	1·38	1·30	54·3 = 100	35
	Young plants	27·83	....	4·13	2·06	1·36	33·82 = 69·2	70
	Increase	....	....	·68	·68	·06	.....	35
	Decrease	11·74	....	....	....	....	20·48 = 31·8	
Cress seed supplied with distilled water, and kept in the sun's rays.	The seed	46·77	1·53	....	3·27	4·8	43·63 = 100	71
	Young plants	37·59	·12	....	4·64	4·75	46·10 = 93·2	121
	Increase	....	....	....	1·37	....	2·47	50
	Decrease	9·18	1·41	....	....	0·05	6·8	
The same seed supplied with river water, and kept more in the shade.	The seed	46·77	1·53	....	3·27	4·8	43·63 = 100	71
	Young plants	33·9	·80	....	4·1	4·9	40·9 = 84·6	121
	Increase	....	....	....	·83	·1	.....	50
	Decrease	12·85	·73	....	....	....	2·73 = 15·4	
Turnip seed supplied with river water, about half of which germinated.	The seed	55·48	3·45	....	3·55	3·1	34·42 = 100	65
	After vegetat.	40·70	1·82	....	4·48	4·48	32·62 = 84·5	109
	Increase	....	....	....	·93	1·38	.....	44
	Decrease	4·78	1·63	....	....	....	1·8 = 15·5	
Rape seed, about two-fifths of which grew.	The seed	55·29	3·45	....	2·71	3·1	35·45 = 100	50
	After vegetat.	44·31	1·58	....	3·14	3·1	40·37 = 92·5	73
	Increase	....	....	....	·43	....	4·92	23
	Decrease	10·98	1·87	....	....	....	..... 7·5	

Table XI.

		Carb.	Hydr.	Oxyg.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Cress seed supplied with river water, and kept in the sun's rays in the greenhouse.	The seed	46·77	1·53	....	3·27	4·8	43·63 = 100	71
	Young plants.	40·03	·98	....	4·44	10·5	36·35 = 92·3	111
Increase		....	....	....	1·17	5·7	....	40
Decrease		6·74	·55	....	....	....	7·28 = 7·7	
The same kind of seeds, but kept in the shade.	The seeds....	46·77	1·53	....	3·27	4·8	43·63 = 100	71
	Young plants	30·08	·92	....	4·42	9·97	33·91 = 79·3	147
Increase		....	....	....	1·15	5·17	....	76
Decrease		16·69	·61	....	....	....	9·72 = 20·7	
Rape seed supplied with river water, and kept in the sun's rays.	The seeds....	55·29	3·45	....	2·71	3·1	35·45 = 100	50
	Young plants	45·35	1·38	....	3·20	8·12	48·75 = 106·8	73
Increase		....	....	....	0·49	5·02	13·3 = 6·8	23
Decrease		9·94	2·07					
The same kind of seeds kept in the shade.	Seed.....	55·29	3·45	....	2·71	3·1	35·45 = 100	50
	Young plants	39·34	1·90	....	3·19	7·52	38·65 = 90·6	82
Increase		....	....	....	·48	4·42	3·2 = ...	32
Decrease		15·95	1·55	....	....	....	.... 9·4	
Mustard seed steeped in river water; kept in the sun's rays.	Seed.....	50·74	2·36	....	3·55	3·9	39·45 = 100	70
	Young plants	33·33	·88	....	3·98	5·0	37·51 = 80·7	119
Increase		....	....	....	·43	1·1	....	49
Decrease		17·41	1·48	....	....	....	1·94 = 19·3	
The same kind of seed kept in the shade.	Seed.....	50·74	2·36	....	3·55	3·9	39·45 = 100	70
	Young plants	31·68	1·66	....	3·93	3·85	30·21 = 71·33	128
Increase		....	....	....	·38	....	....	58
Decrease		19·06	·70	....	....	·05	9·24 = 28·67	
The chemical constitution of the young plants mentioned in Table XI. when brought to 100 parts are	Cress in sun's rays	43·36	1·06	....	4·81	11·4	39·37 = 100	111
	Cress in the shade	37·92	1·16	....	5·57	12·6	42·75 = 100	147
	Rape in sun's rays	42·46	1·29	....	3·00	7·6	45·65 = 100	73
	Rape in the shade	43·39	2·1	....	3·55	8·3	42·66 = 100	82
	Mustard in the sun's rays	41·32	1·09	....	4·93	6·2	46·46 = 100	119
	Mustard in the shade	44·41	2·32	....	5·51	5·4	42·36 = 100	128

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1838.		9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M. from Flint Glass.	Diff. of Wet and Dry Bulb Ther.	External Thermometers. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest	Highest	Rain in inches, &c. from 9 A.M. to 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
JANUARY	M 1	29.978	29.972	48.3	29.924	29.918	49.2	43	02.3	45.7	48.3	43.8	50.2		SSE	{A.M. Fine—light clouds and wind. P.M. Dark heavy clouds. Evening, Overcast—very fine rain.
	T 2	29.888	29.882	47.6	29.794	29.786	48.5	43	01.8	45.8	46.7	43.0	48.7		SE	{A.M. Overcast—very light mist and wind. P.M. Fine—light clouds and wind. Evening, Overcast—deposition.
	W 3	29.642	29.636	47.2	29.692	29.686	47.8	43	02.1	44.3	47.3	43.2	48.8		SSE	{Fine—light clouds, with light brisk wind throughout the day. Evening, Fine and clear.
	T 4	29.976	29.970	44.8	30.006	30.000	45.7	39	01.8	40.2	46.6	39.4	48.3		S	Fine—light clouds & wind throughout the day. Ev. Fine and clear.
	F 5	30.270	30.262	44.0	30.264	30.256	43.3	38	01.2	37.4	36.7	35.8	47.4		W	Thick fog throughout the day. Evening, Foggy.
	S 6	30.292	30.284	40.8	30.242	30.236	41.0	34	01.4	35.5	37.7	32.3	38.7		SW	{A.M. Thick fog—deposition—light wind. P.M. Light fog. Evening, Deposition—light fog.
	⊙ 7	30.304	30.298	40.3	30.306	30.300	40.0	34	02.2	36.7	36.2	35.0	40.4		N	{A.M. Light fog and wind. P.M. Overcast. Evening, Fine and clear—sharp frost.
	M 8	30.398	30.392	34.9	30.336	30.330	34.8	25	Water frozen.	30.6	31.5	28.8	37.3		NE	{A.M. Light snow, with sharp frost. P.M. Overcast—light wind. Evening, Continued frost.
	T 9	30.212	30.204	29.8	30.162	30.156	29.3	23		24.0	23.4	23.0	32.8		N	Overcast—light snow nearly the whole day, with light brisk wind.
	◯ W 10	30.050	30.044	28.8	29.972	29.966	29.7	23		25.3	26.5	23.2	25.6		N	{A.M. Cloudy—light wind, with sharp frost—light snow. P.M. Overcast. Evening, Snow—sharp frost.
	T 11	29.960	29.954	27.3	30.002	29.996	28.4	21		22.4	28.0	19.8	23.2		NW	Overcast—light snow and wind, with sharp frost the whole of the day.
	F 12	30.324	30.316	28.7	30.334	30.326	28.6	20		23.3	26.9	19.7	24.0		NNW	{A.M. Dense fog—light wind. P.M. Overcast—fog cleared. Evening, Fog—sharp frost.
	S 13	30.314	30.306	26.9	30.234	30.228	27.6	21		21.8	25.2	20.8	27.7		SE	{Fine—light clouds and wind, with sharp frost throughout the day. Evening, Overcast.
	⊙ 14	29.988	29.984	27.2	29.916	29.912	28.0	20		24.4	25.2	22.0	26.2		NW	{A.M. Overcast—light snow—brisk wind. P.M. Lightly overcast. Evening, Fine—sharp frost.
	M 15	29.824	29.820	23.6	29.778	29.772	24.3	12		15.7	24.8	14.9	26.3		SW	Overcast—very sharp frost the whole of the day.
	T 16	29.850	29.846	25.8	29.922	29.916	27.2	20		27.0	29.3	11.4	26.3		NW	{Overcast—light snow, with sharp frost throughout the day. Evening, Very sharp frost.
	W 17	30.216	30.208	27.0	30.204	30.198	28.8	21		29.0	30.7	24.2	32.0		NW	{A.M. Overcast—light snow and wind. P.M. Fine—light clouds and wind. Evening, Overcast—light snow—frost.
	T 18	30.016	30.008	26.9	29.910	29.904	26.3	19		23.3	24.2	23.4	31.5		NE	{Overcast—light snow, with light brisk wind throughout the day. Evening, Sharp frost. Ev. Overcast.
	F 19	29.786	29.780	25.3	29.786	29.780	25.2	11		21.0	20.5	21.2	25 6		NNE	A.M. Overcast—lt. snow, with light brisk wind. P.M. Fine—lt. clouds.
	S 20	29.934	29.928	21.2	29.910	29.904	21.7	14		12.8	21.2	11.5	21.0		NW	A.M. Dense fog. P.M. Fine—lt. clouds. Ev. Light fog—sharp frost.
	◯ 21	29.892	29.886	22.4	29.758	29.752	25.0	16		24.5	31.8	13.8	25.0		ENE	{A.M. Fine—light clouds—brisk wind. P.M. Cloudy—light brisk wind. Evening, Fine—light clouds—thaw.
	M 22	29.680	29.676	27.7	29.700	29.696	31.0	24		39.8	42.8	24.7	40.3		ESE	{A.M. Overcast—light wind and rain. P.M. Fine—lt. clouds —continued thaw. Evening, Overcast.
	T 23	29.706	29.700	33.3	29.662	29.658	34.9	33	01.6	34.4	36.4	32.7	43.3		NE	{A.M. Overcast—light brisk wind—continued thaw. P.M. Fine—light clouds. Evening, Overcast.
	W 24	29.752	29.748	30.9	29.738	29.734	29.6	23	Water frozen.	26.7	25.7	26.4	37.4		NE var.	Overcast—very high wind—sharp frost the whole day. Ev. Sharp frost.
	T 25	29.554	29.548	27.6	29.460	29.454	28.6	21		26.7	27.9	24.8	27.0		ENE	Overcast—brisk wind the whole day. Evening, Sharp frost.
	● F 26	29.362	29.356	29.0	29.332	29.326	30.0	24		28.6	29.5	26.4	29.2		ENE	Ditto Ditto Ditto.
	S 27	29.284	29.278	30.2	29.280	29.276	30.8	23		29.8	29.8	28.4	30.4		ENE	Overcast—lt. brisk wind the whole day. Evening, Overcast—frost.
	⊙ 28	29.454	29.450	31.7	29.452	29.448	32.0	24		28.7	31.0	28.5	31.0		E	{Overcast—light fog and wind nearly the whole of the day. Evening, Overcast—light mist and wind.
	M 29	29.464	29.458	33.0	29.510	29.502	35.2	29	01.5	37.8	43.8	28.8	38.4		E	A.M. Overcast—rapid thaw—light fog. P.M. Fine—light clouds —continued thaw. Evening, Overcast.
	T 30	29.640	29.634	37.2	29.648	29.640	37.2	32	00.8	35.8	34.2	35.8	44.3	.072●	E	{Overcast—light fog, with deposition the whole of the day. Evening, Overcast—deposition.
	W 31	29.784	29.778	37.3	29.856	29.850	37.2	31	01.3	35.2	35.3	33.8	36.8	● Melted frost.	NE	Overcast—light wind throughout the day. Evening, Overcast.
	MEAN.	29.897	29.891	32.5	29.874	29.868	33.1	25.9	01.6	30.1	32.4	27.1	34.4	Sum. .072	Mean Barometer corrected	9 A.M. 3 P.M. P. 29.890 .. 29.866 C. 29.883 .. 29.859
FEBRUARY	T 1	30.076	30.070	35.7	30.098	30.092	35.4	29	01.5	32.8	34.0	32.4	36.0		N	{Overcast—light brisk wind with slight frost throughout the day. Evening, Sharp frost.
	F 2	30.324	30.316	35.7	30.344	30.336	36.0	28	00.2	32.3	33.7	31.3	34.6		NNW	A.M. Overcast—lt. wind. P.M. Fine—lt. clds. & wind. Ev. Fine & clear.
	S 3	30.368	30.360	33.7	30.372	30.364	34.8	27	02.5	30.8	33.5	29.5	34.4		NNE	{A.M. Fine—light clouds and wind. P.M. Overcast—light snow and wind. Evening, Fine and clear.
	⊙ 4	30.358	30.350	32.0	30.318	30.312	32.2	21	Frozen.	27.3	32.9	26.5	34.4		NNW	{Fine—light clouds—sharp frost with brisk wind throughout the day. Evening, Fine and clear. Ev. Fine & clear.
	M 5	30.288	30.282	29.8	30.230	30.224	32.3	23		29.7	35.3	26.3	33.2		NE	A.M. Fine—light clouds—sharp frost with brisk wind. P.M. Overcast.
	T 6	30.040	30.034	32.0	29.872	29.866	32.8	25		29.8	34.4	28.2	35.8		ENE	{Fine—light clouds—sharp frost with brisk wind throughout the day. Evening, Hail—high wind.
	W 7	29.514	29.508	34.2	29.382	29.374	36.3	29	03.0	39.3	43.6	29.8	40.0	.208	SSE	{A.M. Overcast—light thaw—brisk wind. P.M. Cloudy—continued thaw and wind. Evening, Rain—high wind.
	T 8	29.112	29.108	40.4	28.964	28.956	42.5	35	02.6	44.2	46.7	38.8	46.0	.033	SW	{A.M. Fine—nearly cloudless—light wind. P.M. Overcast—light rain—high wind. Evening, Overcast—continued rain & wind.
	◯ F 9	28.708	28.702	43.7	28.656	28.652	44.4	39	02.1	43.7	43.6	41.8	48.4	.100	SW var.	Overcast—lt. rain—h. wind the whole day. Ev. Deposition—lt. wind.
	S 10	29.166	29.160	39.8	29.096	29.092	39.6	30	01.9	32.5	35.2	31.6	46.2	.038	NNE	Overct.—sharp frost with lt. brisk wind throughout the day. Ev. Cldy.
	⊙ 11	29.356	29.350	36.2	29.422	29.418	36.5	24	02.8	29.3	35.0	27.3	35 5		NW	{Fine—light clouds and wind with sharp frost throughout the day. Evening, Fine and clear.
	M 12	29.596	29.590	33.6	29.550	29.542	34.2	26		28.4	33.6	26.0	35.3		SW	{Overcast—light fog with sharp frost throughout the day. Evening, Fine and clear—sharp frost.
	T 13	29.534	29.530	31.4	29.536	29.530	32.8	24		26.9	35.0	24.8	34.5		W	{A.M. Fine and clear—sharp frost. P.M. Cloudy—sharp frost. Evening, Cloudy—continued frost.
	W 14	29.764	29.760	31.3	29.718	29.710	32.8	24		27.9	35.3	26.5	35.2		NW	Fine—lt. clouds & wind throughout the day. Ev. Fine—light clouds.
	T 15	29.638	29.632	30.8	29.574	29.568	31.9	25	Water Frozen.	28.5	30.5	27.0	36.2		NE	{Cloudy—light brisk wind with sharp frost throughout the day. Evening, Fine and clear.
	F 16	29.536	29.530	31.3	29.596	29.590	32.6	21		29.9	31.8	28.0	31.6		NE	{Fine—light clouds—brisk wind with sharp frost nearly the whole of the day. Evening, Hazy snow.
	S 17	29.644	29.638	30.8	29.660	29.654	32.0	25		28.6	33.0	26.3	32.5		NE	{Overcast—hail, rain, and snow, with high wind. Evening, Thaw —light rain and wind.
	⊙ 18	30.048	30.042	33.3	30.130	30.126	34.2	28	01.8	33.2	35.0	28.7	35.3	.222	NW	Overcast—thaw with light wind throughout the day.
	M 19	30.156	30.158	34.2	30.048	30.042	36.2	29	01.4	33.5	37.8	32.3	35.5		NE	{Overcast—light brisk wind with slight thaw. Evening, Fine and clear—sharp frost.
	T 20	29.702	29.698	33.7	29.638	29.632	35.8	25	02.2	31.3	39.7	29.0	38.2		E	{A.M. Fine—light clouds and wind. P.M. Overcast—light wind. Evening, Very fine rain.
	W 21	29.706	29.700	36.4	29.704	29.698	37.6	31	01.4	35.3	37.7	31.0	40.4		NNE	Overcast—deposition—light wind throughout the day. Ev. Overcast.
	T 22	29.700	29.694	36.2	29.644	29.640	37.0	30	02.0	34.2	36.9	32.2	38.3		NE	Overcast—light brisk wind the whole of the day.
	F 23	29.478	29.472	37.2	29.378	29.372	37.2	32	01.1	33.8	34.8	33.6	37.4		NE var.	{A.M. Overcast—deposition—brisk wind. P.M. Light rain and wind. Evening, Continued rain and wind.
	● S 24	28.770	28.768	38.6	28.688	28.684	39.9	35	00.6	39.8	44.5	33.5	40.6	.416	ENE	Overcast—light steady rain with brisk wind nearly the whole of the day. Evening, Fine and clear.
	⊙ 25	28.754	28.750	41.5	28.804	28.800	44.0	36	02.9	43.7	48.5	39.7	46.0	.250	SW	{A.M. Cloudy—brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
	M 26	28.950	28 946	41.8	28.974	28.968	41.7	37	01.6	38.8	39.3	38.2	48.7		NE	A.M. Overcast—lt. brisk wind. P.M. Light rain. Ev. Heavy rain.
	T 27	29 136	29.134	40.2	29.148	29.144	40.6	35	01.7	37.7	38.3	35.0	40.4	.333	ENE	{Overcast—light rain with high wind the whole of the day. Evening, Continued rain.
	W 28	29.114	29.108	41.4	29.152	29.148	43.8	36	01.0	42.2	47.8	37.3	43.4	.077	S	{A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Overcast—light rain and wind.
	MEAN.	29.591	29.585	35.6	29.561	29.555	36.7	28.9	01.8	33.8	37.4	31.2	38.4	Sum. 1.677	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.577 .. 29.544 C. 29.570 .. 29.536

METEOROLOGICAL JOURNAL FOR MARCH AND APRIL, 1838.

1838.		9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	Att. Ther.	Dew Point at 9 A.M.	Diff. of Wet and Dry Balls Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	3 P.M.	Self-registering Lowest	Highest	Rain in Inches. Read at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
	T 1	29.124	29.118	44.3	29.104	29.098	45.6	39	01.8	43.7	47.5	41.8	49.2	.133	S	Lightly overcast, with light wind throughout the day. Evening, Light rain.
	F 2	29.094	29.090	45.0	29.122	29.116	45.3	40	01.5	41.7	44.6	40.9	48.0	.088	NE	A.M. Overcast—deposition—light wind. P.M. Overcast—light rain.
	S 3	29.246	29.242	44.9	29.222	29.218	45.7	39	02.0	42.0	44.3	41.4	45.0	.063	W	Overcast—light wind throughout the day, as also the evening.
	⊙ 4	29.030	29.024	42.8	29.000	28.998	43.6	38	00.3	38.5	45.2	37.6	45.3	.105	NE	Overcast—light rain with high wind the whole of the day.
	M 5	29.614	29.606	43.7	29.820	29.814	44.8	37	02.1	41.2	43.7	38.4	47.6	.108	NW	Overcast—light wind throughout the day. Evening, Light rain and wind.
	T 6	29.796	29.792	46.0	29.826	29.820	47.3	40	02.8	46.6	50.8	40.7	47.0	.161	WSW v.	A.M. Fine—light clouds—brisk wind. P.M. Cloudy—brisk wind. Evening, Fine and clear.
	W 7	29.912	29.906	44.8	29.876	29.870	46.4	37	02.1	41.9	49.4	37.0	51.3		W	Fine—light clouds and wind throughout the day.
	T 8	30.116	30.108	43.7	30.200	30.194	45.6	36	03.2	42.2	47.3	37.8	50.0		NW	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
	F 9	30.302	30.296	43.2	30.216	30.208	44.2	37	02.0	37.3	44.2	35.2	47.8		NW	A.M. Thick fog. P.M. Fine—light clouds and wind. Evening, Fine—light clouds.
	S 10	30.000	29.996	42.0	29.922	29.916	44.4	38	02.9	40.3	45.3	35.0	44.9		SE	Fine—light clouds and wind throughout the day.
	○ 11	29.778	29.772	43.2	29.780	29.776	44.0	36	03.1	39.8	45.5	36.0	46.4		E	Fine—nearly cloudless—light wind the whole of the day. Evening, Fine and clear.
	M 12	30.120	30.114	41.8	30.162	30.154	43.3	35	02.2	38.8	46.4	35.9	46.0		N	A.M. Light fog. P.M. Fine and cloudless. Evening, Fine & clear.
	T 13	30.232	30.226	42.2	30.140	30.132	44.2	37	02.8	41.8	48.2	37.0	46.8		SE	A.M. Overcast—light wind. P.M. Light rain. Evening, Overcast.
	W 14	30.062	30.054	46.3	30.006	30.000	49.2	43	03.0	50.6	58.0	41.8	51.4		W	Lightly overcast, with light wind throughout the day. Ev. Cloudy.
MARCH	T 15	29.948	29.944	49.3	29.976	29.970	49.7	43	03.1	46.5	47.3	44.6	58.3		NW	Cloudy—light wind the whole of the day. Evening, Fine & clear.
	F 16	29.900	29.896	48.2	29.892	29.686	49.6	39	03.0	41.8	48.6	37.2	48.4		W	(A.M. Fine—nearly cloudless—light wind. P.M. Fine—light clouds —brisk wind. Evening, Overcast—heavy rain.
	S 17	29.480	29.476	47.0	29.436	29.430	46.8	36	04.8	41.3	45.8	36.3	50.0	.180	NW	Fine—it. clouds—brisk wind throughout the day. Ev. Fine & clear.
	⊙ 18	29.618	29.612	46.2	29.618	29.614	45.0	36	04.0	41.0	45.0	36.2	47.0		W	A.M. Fine—it. clouds & wind. P.M. Cloudy—it. wind. Ev. Overcast.
	M 19	29.610	29.602	44.0	29.618	29.612	45.4	38	02.8	41.2	46.4	39.8	46.0		S	(Overcast—light mist nearly the whole of the day. Evening, Overcast—high wind.
	T 20	29.222	29.216	47.8	29.256	29.250	49.3	42	03.7	50.2	49.4	41.0	51.7		SW var.	Fine—light clouds—high wind, with occasional hail and rain during the day. Evening, Fine and clear.
	W 21	29.394	29.390	50.4	29.338	29.334	46.7	38	05.3	43.7	48.3	36.0	53.3	.055	SSW	(A.M. Fine—light clouds and wind. P.M. Cloudy—high wind. Evening, Fine and clear.
	T 22	29.522	29.516	45.0	29.564	29.560	45.3	37	03.2	40.0	40.4	35.8	49.3		NW	Overcast—brisk wind throughout the day. Evening, Fine & clear.
	F 23	29.580	29.576	44.9	29.518	29.512	43.2	31	03.9	36.3	38.8	31.0	41.8		NW	(A.M. Fine—light clouds—brisk wind. P.M. Overcast—light snow, as also in the evening.
	S 24	29.566	29.560	43.3	29.522	29.516	44.2	35	03.8	41.0	43.5	35.4	42.6		SSW	(A.M. Fine—light clouds and wind. P.M. Overcast—brisk wind. Evening, Light rain.
	● 25	29.820	29.814	49.0	29.932	29.928	45.5	37	05.1	44.8	49.3	36.4	47.2	.036	WSW	Fine—light clouds and wind the whole of the day.
	M 26	30.130	30.124	42.9	30.122	30.116	45.8	35	04.0	42.7	51.3	35.2	50.8		NW	Overcast—light brisk wind the whole of the day.
	T 27	30.312	30.304	45.0	30.310	30.302	46.2	38	03.5	43.9	53.3	38.4	51.4		NE	(A.M. Overcast—light fog and wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
	W 28	30.524	30.518	48.8	30.508	30.500	48.3	40	00.4	43.8	52.0	40.0	54.4		N	(A.M. Fine and cloudless—light wind. P.M. Fine and cloudless— light haze. Evening, Fine and clear.
	T 29	30.530	30.524	46.0	30.506	30.500	48.3	40	01.8	40.3	56.8	38.7	52.5		NE	A.M. Thick fog. P.M. Fine—light clouds and wind. Ev. Overcast.
	F 30	30.488	30.480	50.5	30.370	30.364	51.7	46	04.9	51.8	55.8	40.2	57.0		NNW	(A.M. Overcast—light wind. P.M. Fine—light clouds and haze. Evening, Fine and starlight.
	S 31	30.180	30.172	49.8	30.134	30.128	50.7	43	03.1	45.8	43.8	41.8	56.4		NNW	A.M. Thick haze. P.M. Overcast. Evening, Cloudy.
	MEAN.	29.815	29.809	46.2	29.801	29.795	46.3	38.2	02.9	42.7	47.6	38.1	49.2	Sum. .929	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.772 .. 29.738 C. 29.765 .. 29.731
	⊙ 1	30.204	30.198	51.4	30.164	30.158	44.3	32	02.2	34.5	40.0	30.3	51.6		NNE	(A.M. Fine—light clouds and wind—sharp frost during the night. P.M. Overcast. Evening, Fine and clear—sharp frost.
	M 2	30.056	30.050	46.2	29.914	29.906	43.3	29	01.3	36.3	46.0	29.8	40.8		WSW	Fine—light clouds & wind throughout the day. Ev. Fine—light clouds.
	T 3	29.978	29.972	48.5	29.934	29.926	43.7	33	01.6	39.4	43.4	34.3	47.3		N	(A.M. Fine—light clouds and wind—sharp frost during the night. P.M. Overcast—light wind. Evening, Cloudy.
	W 4	29.928	29.922	47.2	29.942	29.936	47.0	39	03.9	47.8	54.4	39.3	49.7	.016	W	(A.M. Fine—light clouds and wind—rain during the night. P.M. Overcast. Evening, Dark heavy clouds.
	T 5	30.014	30.008	52.2	29.934	29.926	50.3	40	02.4	51.4	56.2	44.9	55.2		W	(The same.
	F 6	29.706	29.700	50.6	29.726	29.720	53.2	45	03.5	52.8	58.3	48.3	56.9	.036	W	A.M. Fine—nearly cloudless—it. wind. P.M. Cloudy—it. wind. Ev. (A.M. Overcast—light wind—rain during the night—overcast the rest of the day. Rain at night.
	S 7	29.550	29.542	52.5	29.326	29.320	53.4	48	03.6	51.7	52.6	50.3	58.8	.033	SW	Overcast—high wind throughout the day, with occasional rain.
	⊙ 8	29.188	29.182	49.7	29.340	29.334	50.6	42	02.9	41.8	48.9	41.5	52.8	.200	W	(A.M. Overcast—light rain with high wind. Fine—light clouds with high wind the rest of the day.
	M 9	29.762	29.756	50.7	29.868	29.862	50.3	37	04.9	43.7	47.6	40.0	49.6		NW	Fine—it. clouds with brisk wind throughout the day. Ev. Fine & clear.
	○ T 10	30.060	30.052	51.2	30.064	30.056	51.6	42	05.8	50.7	58.0	41.2	51.5		SSW	(A.M. Cloudy—brisk wind. P.M. Fine—light clouds—high wind. Evening, Fine—light clouds.
	W 11	30.184	30.178	60.8	30.084	30.078	54.7	46	00.6	54.0	63.4	48.8	59.2		S	A.M. Fine—light clouds & wind. P.M. Fine & cloudless. Ev. Cloudy.
	T 12	30.230	30.222	60.2	30.194	30.188	54.6	44	04.9	49.6	55.7	43.0	64.2		NW	Fine and cloudless—light wind throughout the day. Ev. Fine & clear.
	F 13	30.276	30.270	58.3	30.180	30.172	52.3	40	02.6	47.0	48.5	38.0	56.4		NW	A.M. Fine—light clouds and wind. Overcast the rest of the day.
	S 14	29.986	29.980	49.4	30.002	29.996	49.7	42	04.9	46.2	49.7	43.8	50.8		S	Overcast throughout the day.
APRIL	○ 15	29.792	29.786	49.0	29.692	29.688	51.8	42	02.6	47.0	55.8	44.2	50.3		SW	Overcast—it. brisk wind throughout the day. Ev. Light rain—h. wind.
	M 16	29.718	29.712	51.7	29.664	29.658	49.2	34	06.6	43.5	43.4	37.8	56.8	.050	NW var.	Overcast—high wind throughout the day, with occasional rain & snow.
	T 17	29.628	29.620	49.3	29.648	29.642	46.2	32	05.0	40.8	45.3	33.3	46.7	.025	NW	(A.M. Lightly overcast—brisk wind. P.M. Light rain. Evening, Overcast—hail and rain, with high wind.
	W 18	29.724	29.716	44.2	29.716	29.708	44.6	33	04.6	38.7	44.0	34.4	46.3	.075	NW	(Overcast—light brisk wind throughout the day. Evening, Snow and rain with high wind.
	T 19	29.716	29.708	41.8	29.706	29.698	43.7	35	02.5	36.2	42.8	34.4	44.7	.011	NW	Overcast—it. wind with occasional rain throughout the day. Ev. Hail.
	F 20	29.772	29.766	44.9	29.720	29.712	43.5	34	03.4	41.3	38.3	35.3	44.0		NNW	(A.M. Fine—light clouds. P.M. Overcast—light snow, rain, and wind. Evening, Light rain.
	S 21	29.658	29.652	42.3	29.556	29.550	44.7	35	04.5	41.8	47.4	36.2	44.0	.127	NW	(A.M. Fine—light clouds. P.M. Fine—light clouds and wind. Evening, Fine and clear—high wind.
	○ 22	29.182	29.176	42.7	29.250	29.246	44.2	35	04.4	41.9	45.5	36.8	48.3		E var.	(A.M. Overcast—very light rain—high wind. High wind throughout the night. P.M. Overcast. —it. rain.
	M 23	29.372	29.366	49.3	29.296	29.290	48.8	37	04.6	50.3	52.8	41.0	51.5		S	A.M. Cloudy—it. wind. P.M. Fine—it. clouds & wind. Ev. Overcast
	● T 24	29.460	29.454	47.4	29.566	29.558	48.8	40	03.5	44.8	49.4	40.8	54.7	.055	NE	(A.M. Overcast—brisk wind. P.M. Fine—light clouds and wind. Evening, Light rain.
	W 25	29.740	29.732	51.4	29.756	29.750	49.7	41	03.5	47.0	46.4	38.2	50.6	.036	NNE	(Lightly overcast—brisk wind nearly the whole of the day. Evening, Lightning, with high wind.
	T 26	29.864	29.856	47.6	29.904	29.898	48.9	42	02.6	46.0	44.9	43.0	51.0		NW	Overcast—high wind throughout the day.
	F 27	29.926	29.920	47.8	29.900	29.892	47.8	37	05.0	43.8	46.8	37.8	49.2		NNE	(A.M. Lightly overcast—high wind. P.M. Fine—light clouds and wind. Evening, Fine and clear—high wind.
	S 28	29.840	29.836	51.3	29.764	29.756	46.8	35	05.5	43.8	48.7	35.0	47.7		NNE	(A.M. Fine—light clouds—high wind. High wind throughout the night. P.M. Fine—nearly cloudless. Ev. Overcast.
	○ 29	29.708	29.704	60.3	29.696	29.692	48.0	32	06.7	41.2	46.2	36.2	50.4		NW	(rain.
	M 30	29.546	29.540	45.7	29.506	29.500	48.2	35	05.4	45.7	50.5	38.8	47.0		SE	A.M. Fine—it. clds. & wind. P.M. Ovst.—h. snow & wind. Ev. Very it. (A.M. Overcast—light brisk wind. P.M. Very light rain. Evening, Rainbow visible, with it. rain—rest of evening overcast—it. rain.
	MEAN.	29.792	29.786	49.9	29.767	29.761	48.5	37.9	03.8	44.7	49.0	39.2	50.9	Sum. .664	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.738 .. 29.718 C. 29.732 .. 29.711

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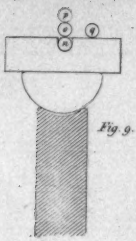
1838.		9 o'clock, A.M. Barometer uncorrected. Flint Glass.	Crown Glass.	Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	Crown Glass.	Att. Ther.	Dew Points at 9 A.M. and 3 P.M.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	3 P.M.	Self-registering. Lowest	Highest	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
MAY	T 1	29.632	29.626	48.4	29.598	29.592	50.3	42	04.0	48.8	53.6	45.3	53.0	.047	SSE	Overcast—light wind throughout the day, with occasional rain.
	W 2	29.840	29.834	65.6	29.800	29.796	57.4	48	06.4	59.3	63.7	48.3	61.3	.111	S	{A.M. Cloudy—light brisk wind. P.M. Fine—light clouds. Evening, Fine—light clouds. Distant thunder with vivid lightning.
	T 3	29.870	29.864	56.6	29.838	29.832	58.4	50	05.1	56.7	61.2	51.7	64.6		S	{A.M. Cloudy—light wind. P.M. Fine—light clouds—brisk wind. Evening, Fine and clear.
	F 4	30.014	30.006	62.9	29.964	29.956	60.4	52	06.4	58.4	65.4	48.8	63.0		NE	A.M. Cloudy—it. wind. P.M. Fine—fl. clouds. Evening, Fine & clear.
	S 5	30.136	30.132	61.8	30.126	30.122	59.9	50	08.0	58.9	60.7	49.4	65.6		NW	{A.M. Fine—light clouds—brisk wind. P.M. Cloudy—rain, with brisk wind. Evening, Fine and clear.
	⊙ 6	30.308	30.296	54.4	30.268	30.264	57.0	47	03.6	47.4	61.0	45.8	64.4	.105	NE	{A.M. Lightly overcast—light wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
	M 7	30.328	30.324	65.4	30.272	30.264	59.2	42	07.0	54.4	72.8	42.3	61.4		N	Fine & cloudless—light wind throughout the day. Ev. Fine & clear.
	T 8	30.328	30.322	64.2	30.262	30.254	61.7	51	08.9	64.2	73.4	50.3	73.2		NE	Fine & cloudless—light wind throughout the day. Ev. Fine & clear.
	○ W 9	30.200	30.198	75.0	30.122	30.116	62.4	50	10.7	59.9	68.3	48.8	73.3		N	{A.M. Fine—very light clouds and wind. P.M. Fine and cloudless. Evening, Lightly overcast.
	T 10	30.346	30.340	63.6	30.370	30.362	57.3	42	09.0	51.3	52.7	43.8	70.3		NE var.	{A.M. Fine—light clouds and wind. P.M. Fine—nearly cloudless—brisk wind. Evening, Fine and clear.
	F 11	30.394	30.390	65.6	30.294	30.288	55.8	38	07.9	49.4	57.0	38.3	55.3		NE	{A.M. Light clouds and wind. P.M. Fine and cloudless. Evening, Fine and clear.
	S 12	30.148	30.144	62.4	30.002	29.994	56.6	41	08.9	53.4	63.8	41.7	58.4		N	Fine & cloudless—it. wind throughout the day. Ev. Lightly overcast.
	⊙ 13	29.686	29.678	54.9	29.638	29.634	57.0	44	04.5	49.7	49.6	46.2	63.6		NW	{Overcast—light wind, with occasional light rain throughout the day. Evening, Cloudy.
	M 14	29.668	29.660	56.4	29.654	29.646	54.3	38	05.2	46.8	49.4	40.4	56.2		NW	Cloudy—light wind throughout the day. Evening, Fine and clear.
	T 15	29.764	29.758	62.8	29.732	29.728	52.8	35	06.6	45.2	50.8	36.2	53.6		N	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
	W 16	29.858	29.850	61.2	29.818	29.810	52.3	32	06.7	47.4	51.7	36.8	54.7		E	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
	T 17	29.812	29.804	50.3	29.770	29.764	51.4	37	05.7	47.3	55.0	38.9	52.8		NE	{A.M. Lightly overcast—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
	F 18	29.866	29.860	64.3	29.886	29.878	53.6	38	09.4	49.3	54.3	39.0	55.4		NW	{Fine—nearly cloudless—light wind throughout the day. Evening, Fine and clear.
	S 19	29.826	29.818	53.0	29.814	29.806	53.3	40	06.6	50.0	57.7	40.9	55.2		E	Overcast—light brisk wind throughout the day. Evening, Cloudy.
	⊙ 20	29.592	29.586	53.2	29.536	29.532	54.8	45	05.1	52.5	56.0	49.6	58.6		E	{A.M. Overcast—brisk wind. P.M. Overcast—light rain. Evening, Continued rain.
	M 21	29.540	29.532	61.7	29.576	29.570	58.3	46	07.1	56.7	60.4	50.2	65.0	.038	S	{A.M. Overcast—light brisk wind. P.M. Fine—light clouds—high wind. Evening, Overcast.
	T 22	29.530	29.522	57.5	29.592	29.586	57.8	50	05.4	54.5	55.3	52.0	60.7		SE	A.M. Overcast—very it. rain. P.M. Overcast—it. wind. Ev. Cloudy.
	● W 23	29.796	29.788	58.8	29.810	29.802	56.9	45	08.0	55.0	56.7	46.8	59.3	.033	NW	A.M. Cloudy—it. brisk wind. P.M. Overcast. Ev. Overcast—it. rain.
	T 24	29.900	29.896	55.0	29.918	29.912	56.9	46	04.2	51.7	56.8	48.3	57.4	.041	NW	{Overcast—light wind throughout the day. Evening, Fine & clear. Rain during the night.
	F 25	30.012	30.004	55.7	29.962	29.976	57.3	47	02.7	49.7	61.2	46.8	57.7		N	A.M. Overcast—it. wind. P.M. Fine—it. cloudsh wind. Ev. Overcast.
	S 26	30.078	30.068	53.3	30.084	30.078	55.8	47	04.3	50.8	55.3	46.2	61.2		NE	{A.M. Overcast—light brisk wind. P.M. Fine—nearly cloudless. Evening, Fine—light clouds.
	⊙ 27	29.978	29.970	58.8	29.864	29.860	55.5	44	06.1	51.4	56.5	42.6	59.8		NE	A.M. Overcast—light brisk wind. P.M. Fine—light clouds—brisk wind. Evening, Fine and starlight.
	M 28	29.560	29.554	54.2	29.574	29.568	56.9	50	00.7	53.4	59.0	48.2	56.7	.077	ENE	{A.M. Overcast—light rain. P.M. Overcast—light wind. Evening, Fine and clear. Rain during the night.
	T 29	29.628	29.624	67.7	29.670	29.664	61.2	52	07.7	60.2	64.8	51.6	69.7	.166	SSE	A.M. Cldy—it. wind. P.M. Fine—it. clds—h. wind. Ev. Fine & clear.
	W 30	29.808	29.800	62.2	29.838	29.832	61.8	51	06.5	59.3	66.4	52.8	70.4	.013	S	{A.M. Lightly overcast. P.M. Fine—light clouds and wind. Evening, Fine and clear.
	T 31	29.924	29.920	67.8	29.928	29.920	63.8	53	07.5	62.2	68.2	52.2	67.0		S	{A.M. Fine—light clouds and wind. P.M. Lightly cloudy. Evening, Thunder storm, with lightning and heavy rain.
	MEAN.	29.915	29.909	59.8	29.890	29.884	57.0	44.9	09.2	53.4	59.3	45.8	61.3	Sum. .571		Mean Barometer corrected ..... 9 A.M. 29.835 .. 29.816 / 3 P.M. C. 29.837 .. 29.811
JUNE	F 1	29.974	29.970	66.0	29.920	29.912	63.7	56	07.8	60.8	61.3	52.7	70.3	.372	NW	{A.M. Fine—light clouds, P.M. Overcast—heavy rain. Evening, Continued rain.
	S 2	29.836	29.830	62.6	29.780	29.772	64.2	55	06.4	59.3	65.7	52.5	70.3	.644	S	A.M. Lightly overcast—it. wind. P.M. Heavy rain. Ev. Fine & clear.
	⊙ 3	29.800	29.796	64.7	29.754	29.750	64.0	55	07.4	59.5	63.9	51.3	66.0	.341	S	A.M. Cloudy—heavy rain—it. wind. P.M. Overcast. Ev. Cloudy.
	M 4	29.732	29.728	67.0	29.740	29.734	63.7	54	08.1	61.5	65.3	53.6	66.6	.055	S	{Fine—light clouds and wind, with occasional showers. Evening, Fine and clear.
	T 5	29.924	29.920	67.8	29.906	29.900	64.2	54	07.2	58.6	67.8	50.4	70.5	.022	SSW	Cloudy throughout the day.
	W 6	30.052	30.044	59.8	30.100	30.096	60.4	52	03.8	52.7	55.5	51.0	69.3		NW	Overcast—light wind throughout the day. Ev. Fine and clear.
	T 7	30.212	30.204	60.6	30.162	30.154	61.0	51	06.1	54.9	62.5	50.6	58.7		NW	Cloudy—light wind throughout the day. Evening, Fine and clear.
	○ F 8	30.304	30.298	79.3	30.278	30.270	61.6	42	09.1	51.7	57.3	44.2	64.3		NW	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
	S 9	30.240	30.234	64.6	30.128	30.120	61.8	48	08.0	59.0	63.4	47.0	70.3		SW	{A.M. Fine—light clouds and wind. P.M. Cloudy—light wind. Evening, Cloudy.
	⊙ 10	29.798	29.792	58.9	29.682	29.678	61.0	50	07.8	59.3	62.0	50.5	64.2		S	{A.M. Cloudy—light brisk wind. P.M. Overcast—light wind. Evening, Heavy rain.
	M 11	29.518	29.512	58.6	29.538	29.532	61.0	53	03.7	55.2	54.7	52.3	63.0	.227	NE	Overcast—light wind, with heavy showers the whole of the day.
	T 12	29.564	29.556	60.0	29.600	29.594	61.3	54	05.5	55.7	60.0	52.4	60.8	.533	S	{A.M. Dart heavy clouds—rain early. P.M. Heavy rain—hail and thunder. Evening, Fine & clear.
	W 13	29.682	29.676	59.3	29.680	29.674	60.6	53	05.3	56.8	61.3	50.8	62.8	.366	NW	{A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Overcast—heavy rain.
	T 14	29.744	29.736	60.7	29.780	29.774	63.0	54	06.8	59.8	63.4	51.2	63.0	.100	S	A.M. Fine—it. clouds & wind. P.M. Cloudy—brisk wind. Ev. Lt. rain.
	F 15	29.784	29.776	63.6	29.728	29.722	62.8	55	06.1	60.7	61.4	54.0	64.5	.025	S	Overcast—light wind throughout the day, with occasional light rain.
	S 16	29.800	29.792	61.2	29.836	29.828	64.3	54	06.2	61.8	69.3	54.8	65.6	.013	S	Lightly overcast—light wind throughout the day. Ev. Light rain.
	⊙ 17	29.888	29.882	64.5	29.858	29.854	66.6	60	05.4	63.5	70.6	60.3	70.4	.172	S	Overcast—light wind the whole of the day. Rain during the night.
	M 18	29.682	29.678	68.9	29.576	29.572	69.2	62	06.2	68.3	69.8	61.0	71.5	.022	ENE	{A.M. Overcast—brisk wind—thunder and lightning, with heavy rain. P.M. Dark clouds—high wind. Evening, Light rain.
	T 19	29.826	29.820	70.8	29.872	29.866	67.3	58	08.2	63.8	66.4	54.7	71.7	.177	S	A.M. Fine—it. clouds & wind. P.M. Cloudy—it. wind. Ev. Overcast.
	W 20	29.736	29.728	63.8	29.708	29.704	65.2	60	04.9	61.0	64.4	56.2	67.3	.283	SW	Overcast—light rain and wind throughout the day.
	T 21	29.590	29.584	63.9	29.692	29.686	65.6	58	05.2	60.8	63.8	59.0	67.2	.033	W	{A.M. Lightly overcast—brisk wind. P.M. Overcast—heavy shower. Evening, High wind.
	● F 22	29.860	29.854	66.6	29.880	29.872	65.0	57	07.1	62.4	64.8	53.6	72.3		SW var.	{A.M. Dark heavy clouds—brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
	S 23	30.192	30.186	74.6	30.148	30.142	66.3	56	07.3	62.4	70.4	51.7	72.6	.227	NW	Fine—very light clouds the whole of the day. Ev. Fine and clear.
	⊙ 24	30.052	30.048	75.5	29.996	29.992	68.0	59	09.3	66.7	76.5	54.8	79.7		E	Fine—nearly cloudless—it. wind throughout the day. Ev. Fine & clear.
	M 25	30.090	30.082	70.0	30.072	30.066	70.4	61	08.1	68.3	71.8	61.6	77.3		NNW	Cloudy—light wind the whole of the day. Ev. Overcast—light rain.
	T 26	30.000	29.994	65.3	29.992	29.984	66.3	60	03.1	57.4	61.0	56.0	72.8	.572	N	{A.M. Overcast—light rain—heavy rain during the night. P.M. Overcast. Evening, Very light rain.
	W 27	30.044	30.036	69.2	30.038	30.030	66.8	59	07.9	65.0	61.8	57.0	74.6	.019	SW	A.M. Cloudy—very light rain and wind. P.M. Overcast—light wind.
	T 28	30.050	30.044	65.0	30.014	30.006	66.8	58	06.6	62.9	65.6	57.3	66.3		S	Overcast—light wind throughout the day.
	F 29	29.998	29.992	75.7	30.004	29.996	67.2	57	09.0	62.8	59.0	54.3	72.8		W	{A.M. Fine—light clouds and wind. P.M. Overcast—thunder—light rain. Evening, Light rain.
	S 30	29.974	29.970	74.8	29.914	29.906	66.0	56	09.1	62.8	66.3	52.0	72.7	.169	S	{Fine—light clouds and wind throughout the day. Evening, Overcast—light rain.
	MEAN.	29.898	29.892	66.1	29.879	29.873	64.5	55.4	06.8	60.5	64.2	53.6	68.6	Sum. 4.372		Mean Barometer corrected ..... 9 A.M. 29.809 .. 29.787 / 3 P.M. C. 29.795 .. 29.780



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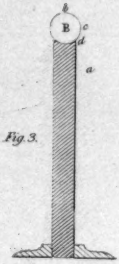

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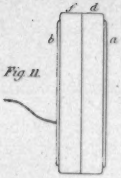

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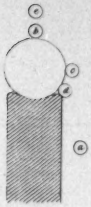

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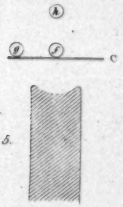


Fig. 5.

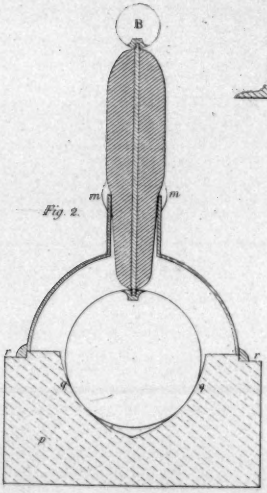


Fig. 2.

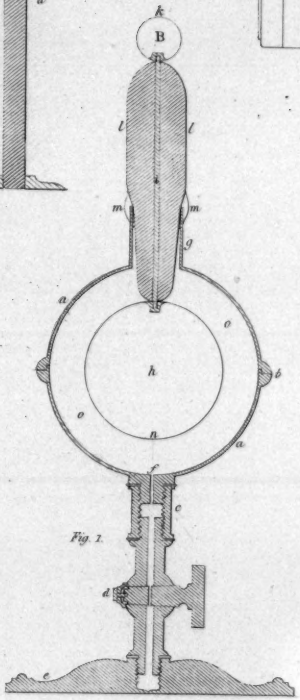


Fig. 1.

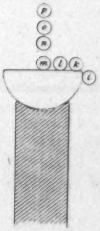
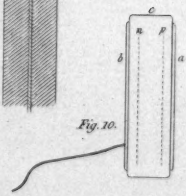


Fig. 7.

Fig. 6.



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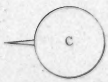


Fig. 8.

Fig. 1.

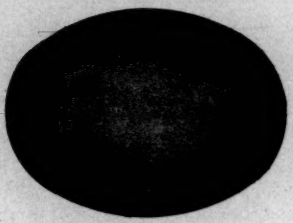
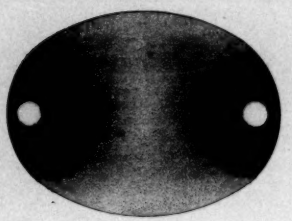


Fig. 2.



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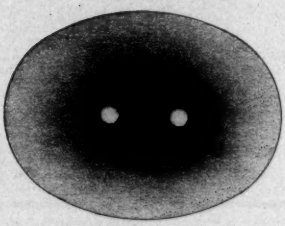
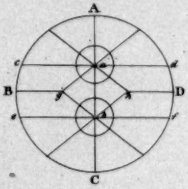


Fig. 4.



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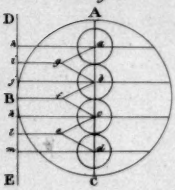

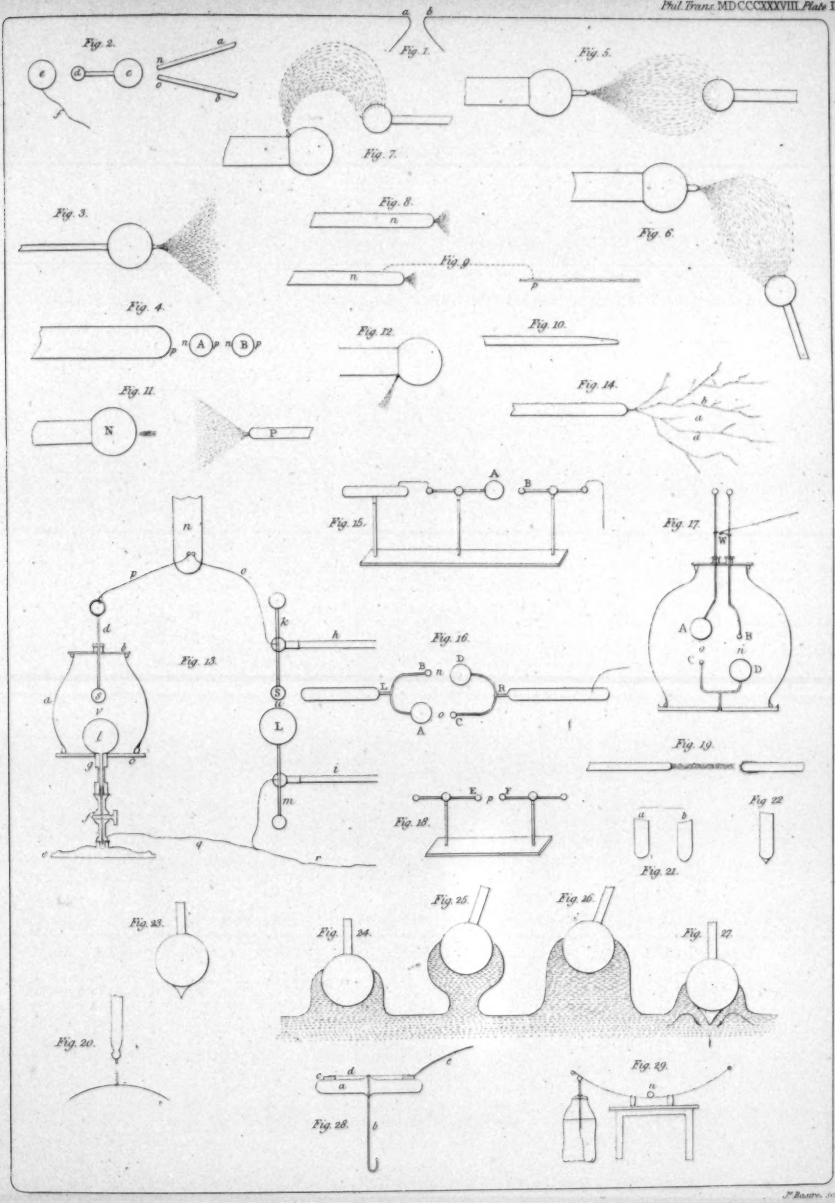
Fig. 1.
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Fig. 29.

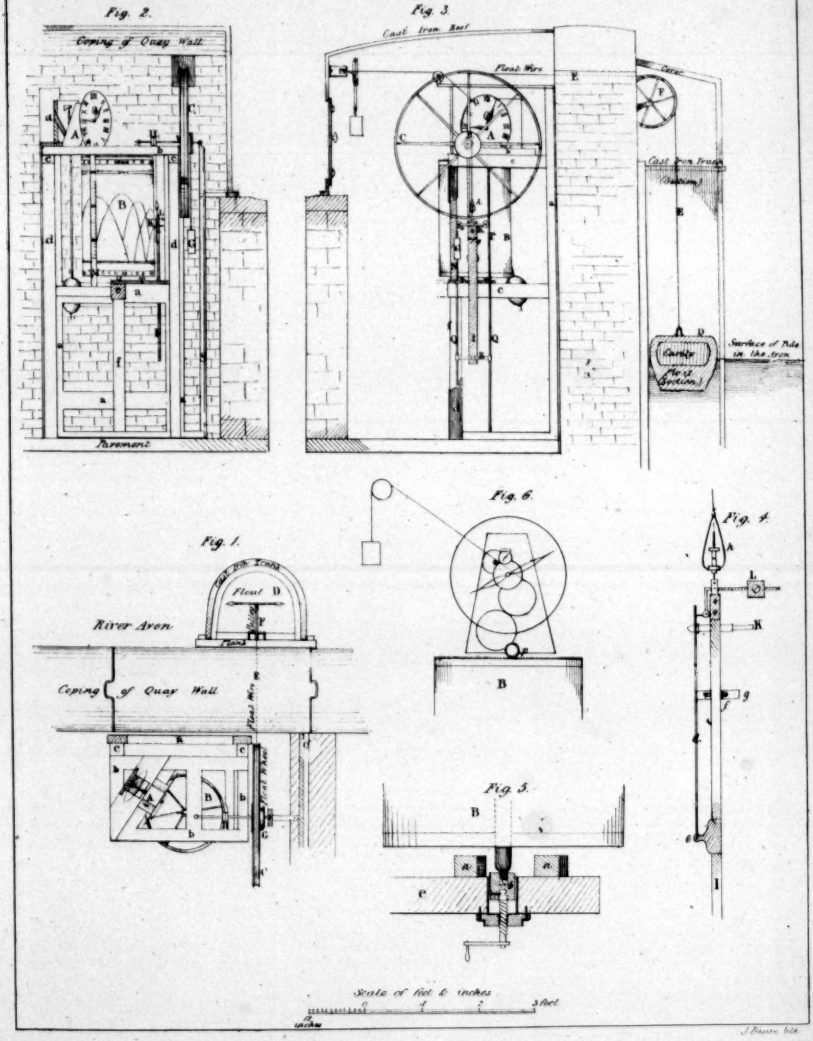
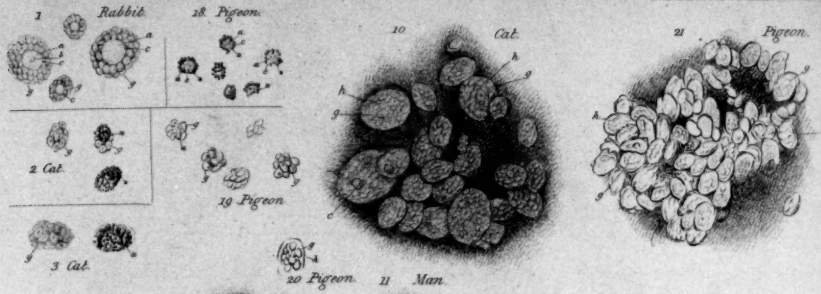
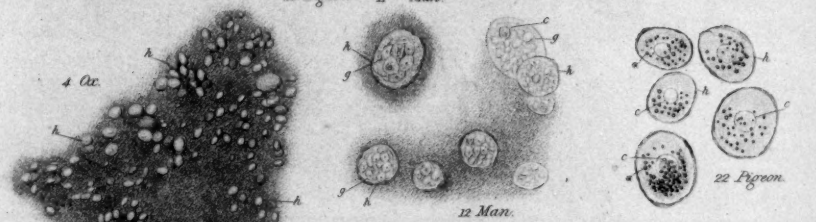
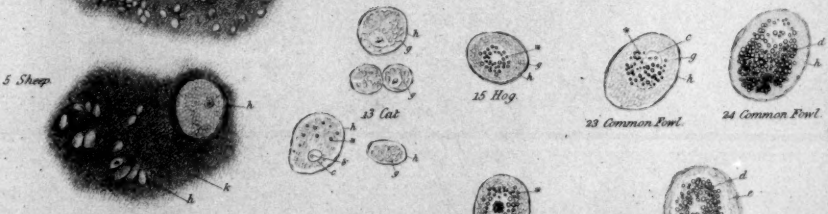
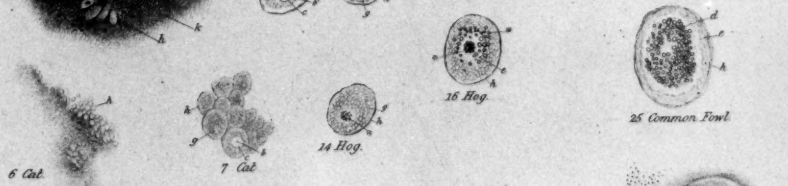


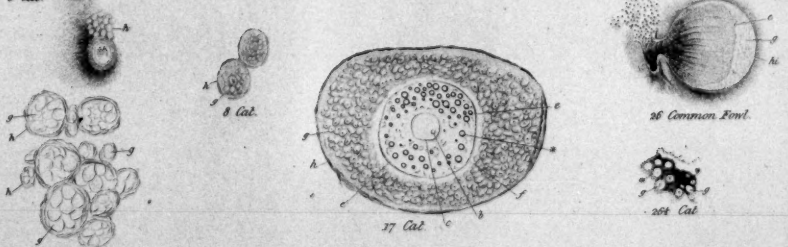
Fig. 2.
Coping of Quay Wall
Pavement
Fig. 3.
Cast Iron Rod
Float Wire
Case
Cast Iron Frame
Carriage
Surface of Tide in the Iron Frame
Fig. 1.
Cast Iron Frame
Float D
River Avon
Coping of Quay Wall
Float Wire
Fig. 6.
B
Fig. 4.
Fig. 5.
B
Scale of feet & inches
3 feet
J. Basire, lith.


1 Rabbit.
18. Pigeon.
10 Cat.
21 Pigeon.
2 Cat.
19 Pigeon.
3 Cat.
20 Pigeon.
11 Man.


4 Ox.
12 Man.
22 Pigeon.


5 Sheep.
13 Cat.
15 Hog.
23 Common Fowl.
24 Common Fowl.

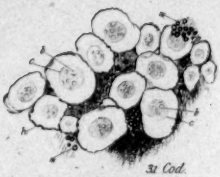

16 Hog.
25 Common Fowl.
6 Cat.
7 Cat.
14 Hog.

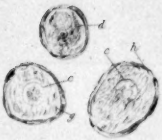

8 Cat.
26 Common Fowl.
9 Dog.
17 Cat.
26* Cat.

Embryology.

27 Frog (R. Wagner)

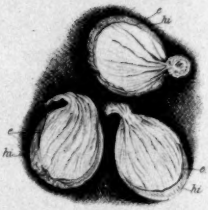
30 Salmon.


31 Cod.


32 Whiting.

27* Colubar Natrix. (R. Wagner)

33 Haddock.


37 Salmon.

34 Turbot.

28 Frog.

36 Salmon.

38 Salmon.

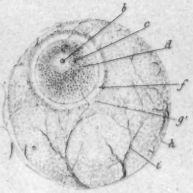
35 Salmon.

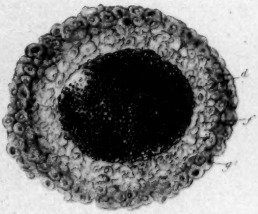
29 Frog.

38* Bird &c. (Ideal)

39 Dog.

40 Mole.


41 Mole.


44 Dog.

42 Mole.

43 Sheep.

45 Hog.

46 Hog.

47 Dog.

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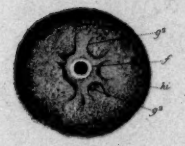
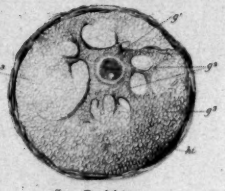
48 Dog.

49 Guineapig.



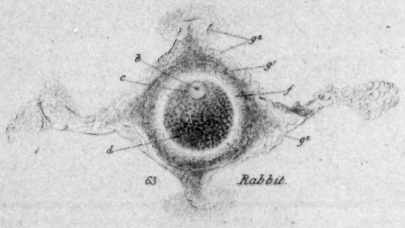
51 Sheep.



50 Rabbit.

52 Ox.

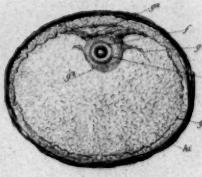
53 Ox.



63 Rabbit.

54 Dog.

57 Dog.



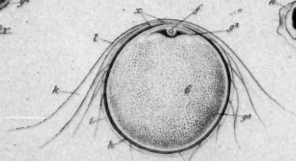
55 Ox.

59 Rabbit.

61 Ox.

60 Ox.

58 Ferret.



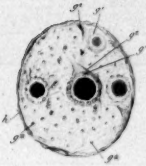
62 Hog (According to Baer.)

56 Ox.

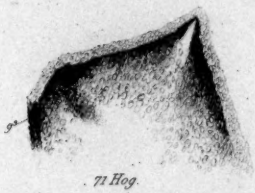
67 Rabbit.

64 Hog.

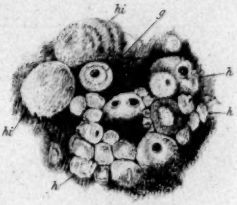
65 Mouse.



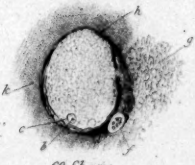
66 Dog.



71 Hog.



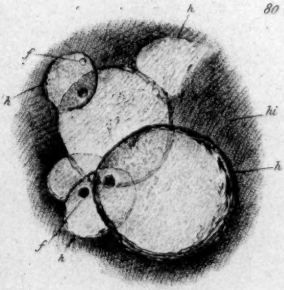
68 Dog.



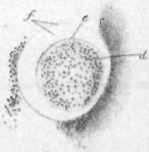
69 Sheep.

72 Man. (Bernhardt.)

80 Ox. (According to Baer.)



74 Hog.



70 Cat.

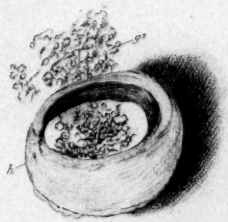
75 Dog.

73 Ox.

76 Pigeon.

77 Ox.

78 Ox.



79 Ox.

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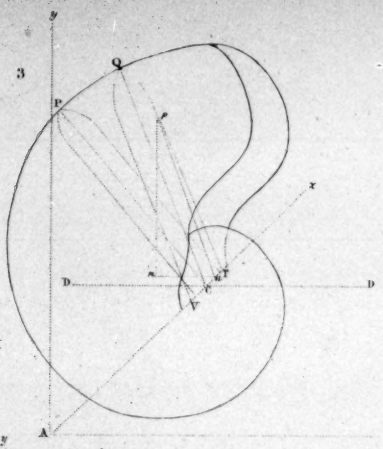
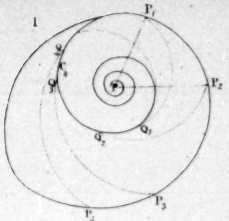

3
y
Q
P
x
D
D
A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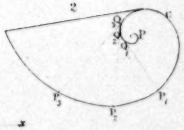
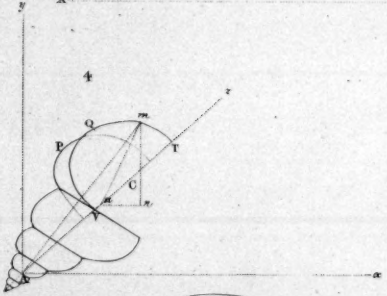
F1
F2
F3
Q1
Q2


2
C
P
Q
F3
F2
F1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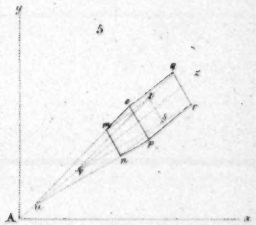
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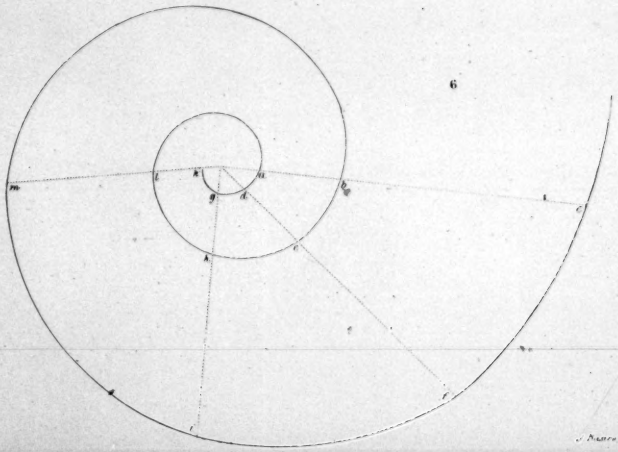
6
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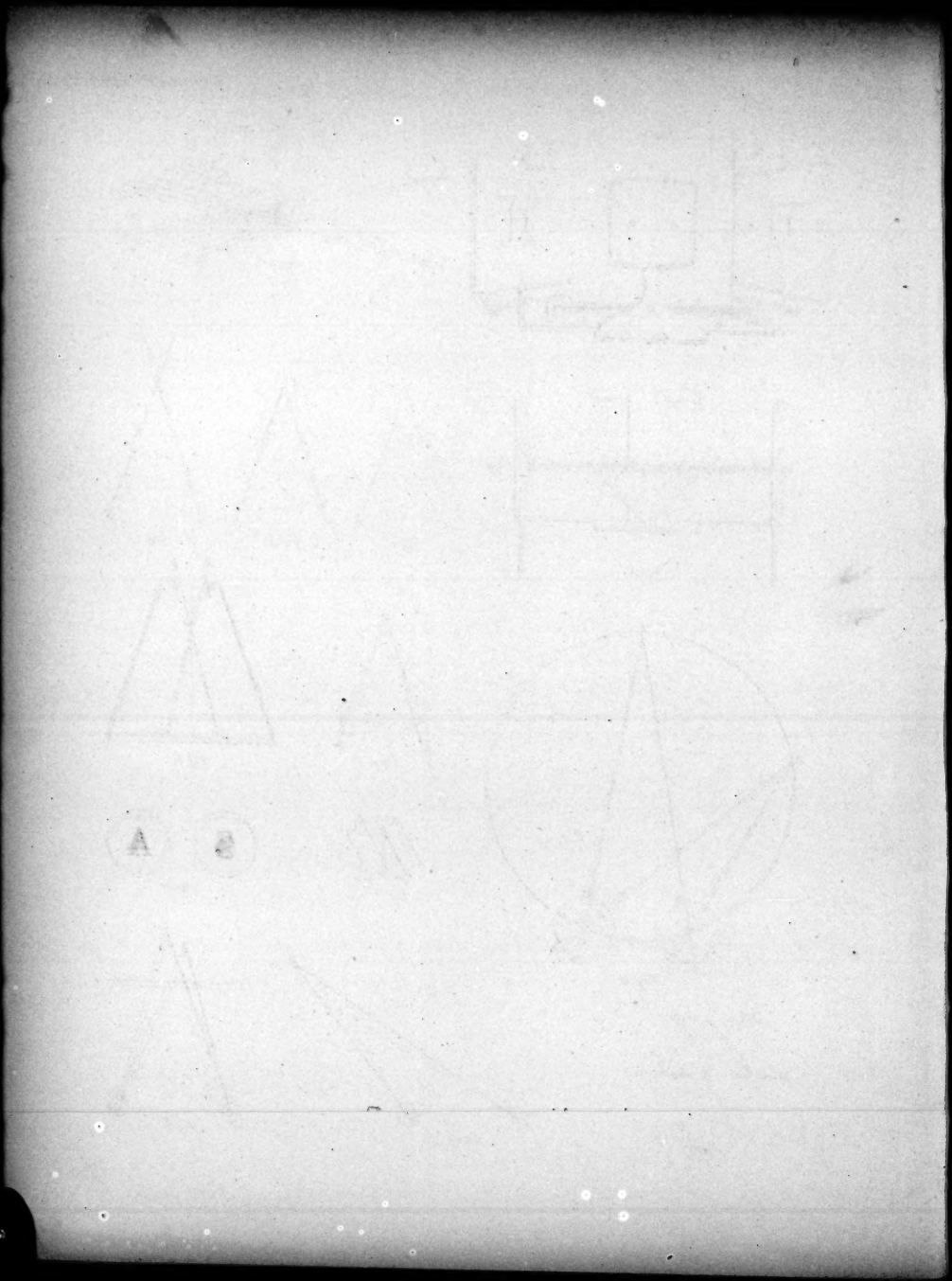
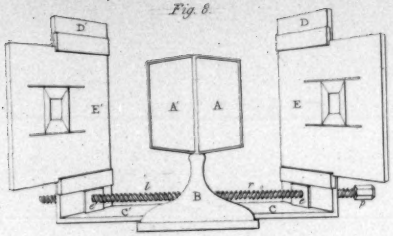
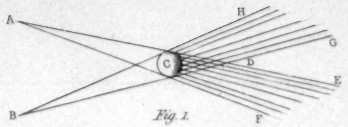


Fig. 8.





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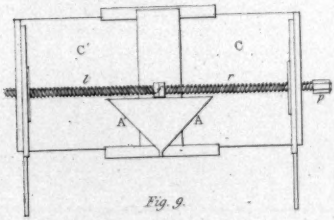


Fig. 9.

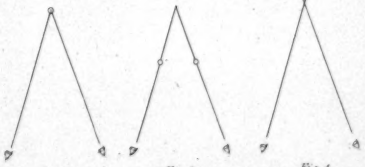
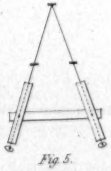


Fig. 2. Fig. 3. Fig. 4.



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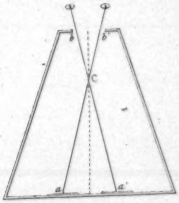

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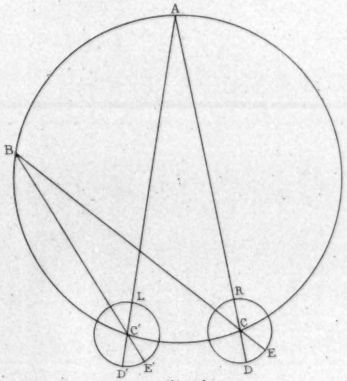
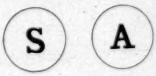


Fig. 26.

Fig. 22.



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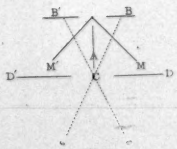

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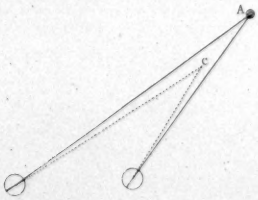

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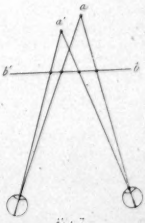

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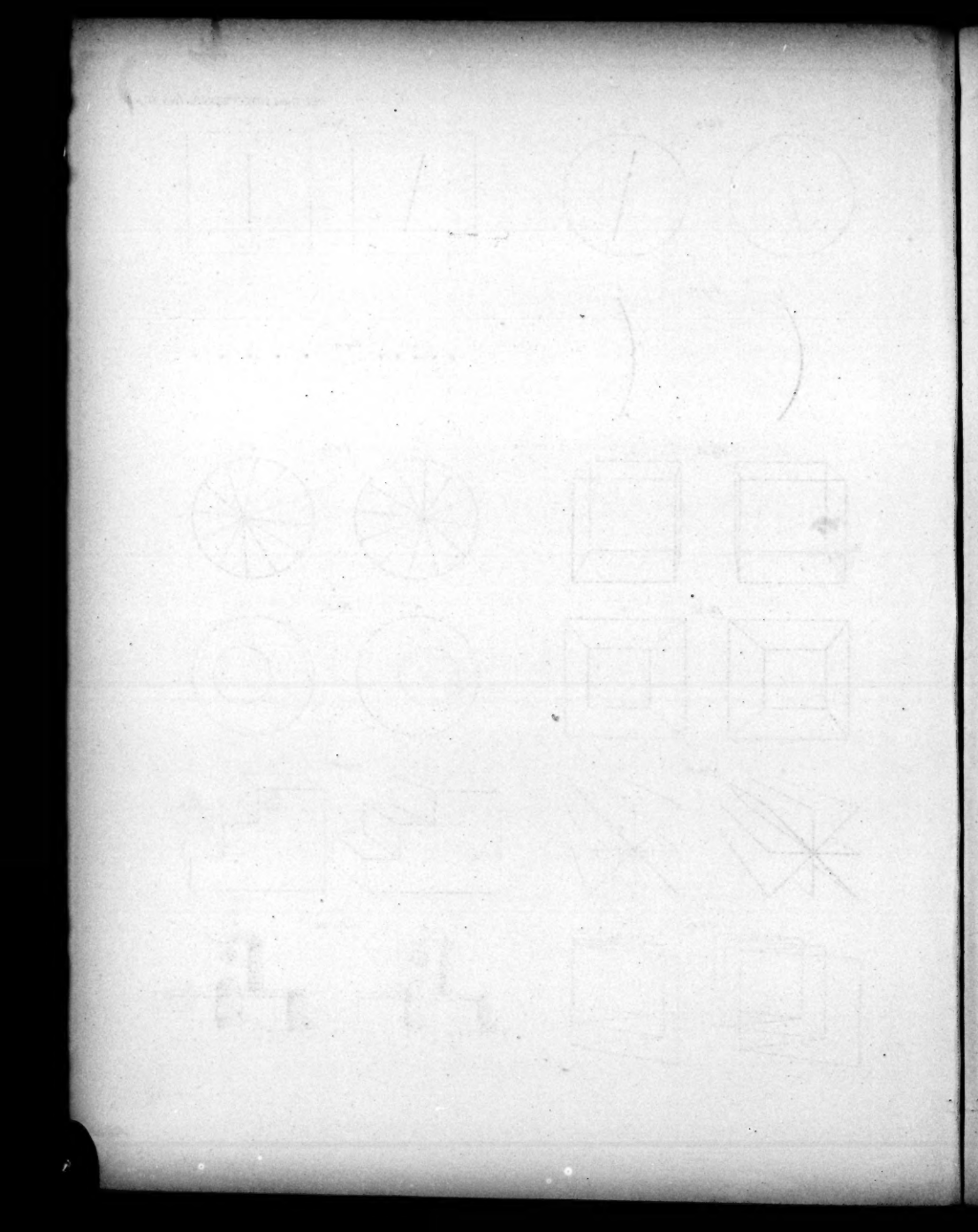

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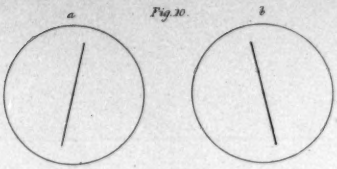

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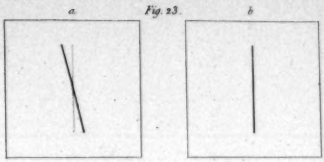

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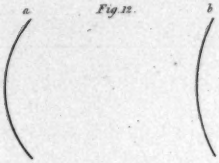

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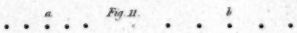

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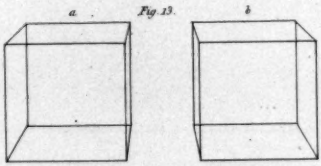

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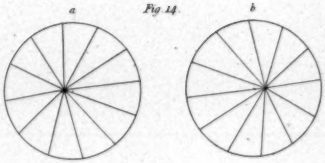

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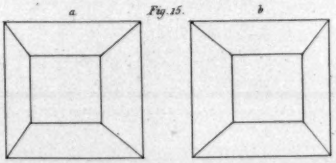

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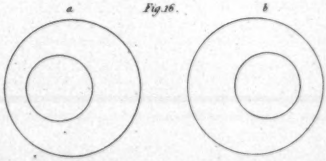

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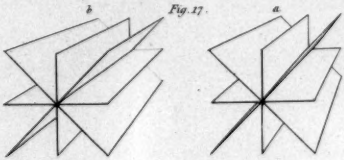

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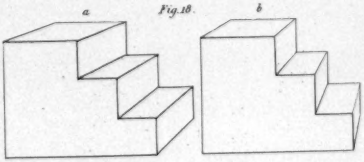

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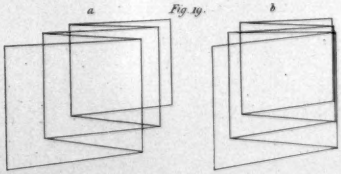

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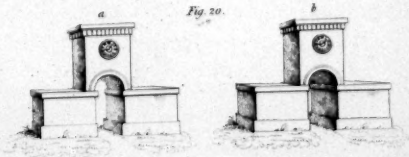

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